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ART. I.— *The Anterior Suspensory Apparatus for the Treatment of Fractures of the Lower Extremity.* By N. R. SMITH, M. D., Professor of Surgery in the University of Maryland.

IN the treatment of all fractures of the extremities, the general indication is to furnish mechanical support that shall supersede the office of the injured bone in maintaining the form and length of the limb, until union shall have been accomplished. The accomplishment of this object is attempted by the use of rigid materials, termed splints, formed of wood, iron, felt, gutta percha, or starched pasteboard and bandages.

There would be no difficulty in effecting the mechanical purpose in view, if we might treat the member as an insensible substance, capable of enduring pressure without the production of pain, sloughing, or ulceration; and if it were possible, at the same time, to maintain one unvarying posture of the trunk. But the living parts cannot long endure the pressure of any hard material, nor the continued constriction of a band; nor is it possible that the trunk can be kept in one unvarying posture.

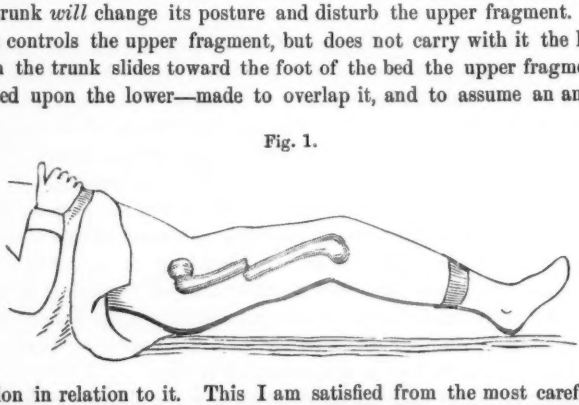
Another source of embarrassment arises from the difficulty of furnishing a well-adapted surface of support for the limb. This is not effected by supplying soft and yielding supports, such as pillows of down or feathers, or other soft bolsters, the name of which suggests comfort, but by no means furnishes it. As the member sinks into such supports, whatever part first touches the surface receives the greatest degree of pressure; and besides, such supports yield, and allow in the limb a change of posture in relation to the trunk. They are also otherwise objectionable, particularly in compound fractures, as causing too much heat, rendering it difficult to change dressings and to preserve cleanliness.

It is the duty of the surgeon to study carefully the physiological and

mechanical causes of displacement, and to obviate them by posture and support, rather than by severe mechanical restraint.

There is a source of deformity and shortening, especially in fractures of the thigh, which has been by surgeons surprisingly overlooked. When the patient has been placed in the ordinary posture in bed, with a pillow and bolster under his head and shoulders, there is a constant tendency in the trunk to slide toward the foot of the bed. This tendency occurs in all cases in which the patient cannot exert his own muscular strength to correct his position. It occurs in typhoid fevers and other diseases inducing great debility, and manifestly must, and does, in fractures of the thigh and leg. The trunk also otherwise changes its position in relation to the limb. This is partly attributable to the yielding of the bed, and partly to the utter impossibility of maintaining the trunk in one immutable posture without the most intense suffering. The patient will instinctively change his posture in sleep, and will unconsciously do it when awake. If, then, the limb rests on the bed, and is embraced in a cumbrous apparatus, which also rests on the bed, it becomes incapable of obeying the motions of the body. The more firmly fixed the limb is in the apparatus, and the more immovable its posture, so much the worse in regard to the support of the fragments. The trunk *will* change its posture and disturb the upper fragment. The trunk controls the upper fragment, but does not carry with it the lower. When the trunk slides toward the foot of the bed the upper fragment is jammed upon the lower—made to overlap it, and to assume an angular

Fig. 1.



position in relation to it. This I am satisfied from the most careful observation is a much more fruitful source of shortening and deformity than is the contraction of the muscles, so constantly referred to by surgeons.

The objects to be attained, then, in the treatment of fractures of the lower extremity are: 1st. To furnish a surface of support which shall be accurately and permanently adapted to the surface and form of the limb which reposes in it. 2d. To so adapt the surface of support as that the limb, sinking into it, shall maintain its form by its own weight, without the necessity of lashings here and there, to secure it to the rigid portions of the apparatus. 3d. To make the limb obedient to all the unavoidable movements of the trunk, so that the upper fragment shall not be jammed

upon the lower, nor contorted in relation to it by the movements of the trunk. 4th. To obviate the contraction of the muscles by the employment of an extending force, uniform in its action, easily graduated, and not requiring the application of bands to the ankle. 5th. So to arrange the supports that in compound fractures we may have free access to the seat of injury without removing any of the essential supports of the limb. 6th. To effect these objects by an apparatus simple in construction, capable of being anywhere procured, easy of application, not requiring to be readjusted, and of but trifling expense.

That these objects are perfectly accomplished by the apparatus here presented I have demonstrated in the clearest manner in hospital and private practice during many years past.

The apparatus which I am about to describe is applicable to all fractures of the thigh and leg. I shall first describe it as employed in fractures of the thigh.

A single splint constitutes the whole of the rigid, or supporting, part of the apparatus. It may be made of wood or wire. The first employed by me was of wood. It was three inches broad, half an inch thick, and long enough to extend from above the spine of the ilium to the toes. It had an angle corresponding to that of the ankle, another at the knee more obtuse, allowing the leg to be very slightly flexed, and a third corresponding to the hip, still more obtuse, slightly flexing the thigh on the pelvis. Two staples by which to suspend it were attached to the upper surface, one a little above the knee and the other about the middle of the leg. It is represented in the plate. The splint may be sawed, in one piece, out of thick pine plank, or may be made of three pieces, united at the angles by nails or screws.

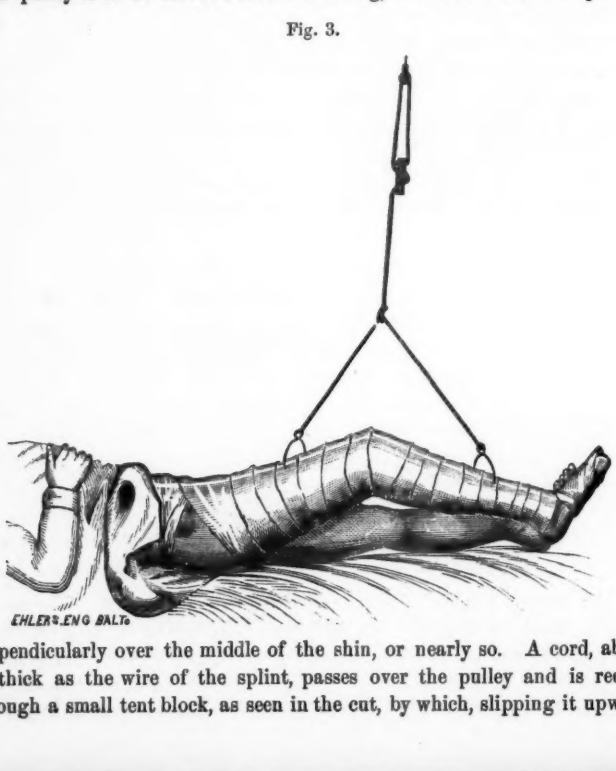
Fig. 2.

I now employ a splint of wire, as represented in the plate. The size of the wire should be that of a No. 10 bougie. If lighter than this it is apt to spring too much. It is constructed of one piece of wire bent twice at right angles at each extremity, in the form represented in the cut—that of a long parallelogram—being, however, three inches wide at its upper ex-

tremity and two and three-fourths at its lower. It must be long enough to extend from a point a little above the anterior spinous process of the ilium, to an inch beyond the toes when the leg, foot, and thigh are extended. Three feet eight inches will be sufficiently long for most adults. A little excess of length, above or below, is unimportant. The side-pieces are to be sustained by cross-pieces at distances of about eight inches, firmly clinched upon them. The wire frame is then to be bent by the surgeon to suit the case. The lower angle corresponds to the ankle, and is one of about 120°, to secure an easy posture of the foot. The angle at the knee is very obtuse—about 160°. The angle at the hip should be of about the same degree. The angle at the ankle should be about six inches from the extremity; that of the hip seven. The middle bend corresponds to the knee. The angles are easily made by bending the splint over the margin of a strong chair or table. It will often be necessary to vary these angles to suit particular fractures. The wire frame is now to be tightly wrapped with a muslin bandage, and it is ready for application.

The suspensory apparatus is simple and easy of application. A small iron pulley is to be screwed into the ceiling, over the bed of the patient,

Fig. 3.



perpendicularly over the middle of the shin, or nearly so. A cord, about as thick as the wire of the splint, passes over the pulley and is reeved through a small tent block, as seen in the cut, by which, slipping it upward

or downward, we elevate or depress the limb. The eccentric pressure prevents the weight of the limb from causing it to slip; if not, rub the cord with chalk. This single cord, which depends from the block, has a loop at its end, about two feet or more above the limb. Through this another cord, about five feet long, passes and hangs double from the loop, by its centre. Each end has a double hook attached to it, of the form represented in the cut. It is made of much smaller wire, has an expansion nearly equal to the breadth of the splint, and has the points sharpened and abruptly turned inward, the bends being quite angular, so as snugly to embrace the wire and not slip upon it. When everything is ready for suspension, these hooks are to be applied to the wire frame, piercing with them the muslin cover of the splint, at points to be presently indicated.

The application of the apparatus is extremely simple. The limb (we are now supposing a fracture of the thigh) is to be carefully adjusted and laid on pillows, still supported by the hands of assistants. The splint is now to be laid along the upper surface of the limb, the foot portion an inch remote from the instep, the middle angle nearly corresponding to the knee. I formerly placed a long narrow compress under the splint, but it is superfluous. One may, however, be placed on the instep, and one under the hip extremity of the splint. I now pass a piece of bandage, long enough to embrace the limb and overlap, under the foot and over the splint, so as to embrace both, not tightly. This band I besmear with starch. Formerly I used adhesive plasters. I pass another around the ankle, another beneath the knee, a fourth above the knee, and a fifth around the thigh near the hip. These bands are to be secured over the splint by pins. The starching of the bands obviates their slipping when traction is made on the splint, as presently to be described.

The limb being still kept extended by the hands of assistants, the hooks are now to be applied. It is very important to select properly their points of attachment, so as to adjust well the centre of gravity of the limb. In fractures of the middle of the thigh, I attach the upper double hook nearly over the seat of the fracture; the other a little above the middle of the leg. By pulling upon the tent-block the limb is now gently raised from the pillows and hangs suspended in the slings. Observe now whether the upper portion of the splint presses down too much on the thigh, or springs up too much from it. If the former occurs, then attach the upper hooks higher; if the latter, attach the lower ones lower. But this may be corrected, if slight, by slipping the cord, with the hooks attached, through the loop of the single cord, so as to correct the bearings, and securing it with a piece of twine just below the loop, to keep it thus.

The limb being now raised, so that one may pass his hand under every part of it, the application of the roller is made. The surgeon commences with the foot, the splint being held steady. Three or four turns are to be made loosely around the foot, care being taken not to press it upward

against the splint, which it must never touch; then figure of 8 turns are made around the ankle and foot, and the roller is carried upward along the leg and thigh, reversing, where necessary to make perfect adaptation, and great care being taken not to crowd the limb too firmly against the splint. On reaching the hip with the roller, a few spica turns are to be made obliquely through the groin and around the pelvis, concluding with two direct turns around the pelvis. I often besmear the whole under surface of the bandage, thus applied, with starch. This prevents the slipping of the bandage and preserves a uniform and accurately adapted surface of support.

The whole is now accomplished, except the application of the extending and counter-extending forces. This is in a moment effected by moving the bedstead of the patient head-ways (a word not found in Worcester), so far as to give a slight obliquity to the suspending cord, inclining toward the foot. It needs generally to deviate not more than one or two degrees from a perpendicular, but may be graduated to any degree of traction by simply moving the bed. Severe traction is not necessary, because the limb is supported in a manner to obviate the principal cause of retraction.

Counter-extension is made by the *vis inertia* of the body. To obviate the descent of the trunk toward the foot of the bed, caused by posture, and slightly promoted by the traction of the oblique cord, there may be a block, two inches thick, placed under each footpost of the bed. Under the upper portion of the thigh I usually place a small pillow, to obviate the lateral swinging motion which the unavoidable movements of the trunk may cause. The limb should be supported just high enough to lift the heel clear of the bed.

Let us now observe to what extent we have accomplished the indications which we proposed above.

1st. It will be seen that the member rests entirely on the turns of the bandage, and these are necessarily perfectly adapted to the form of the inferior surface of the limb. The limb cannot change its position in relation to this surface, even when the trunk moves and carries the member with it. So equally diffused is the pressure that the patient does not feel the contact of the support at any point.

2d. The apparatus being secured to the pelvis, the limb is obedient to all the movements of the trunk, and always preserves the same position in relation to it. Slung by the cord, it yields to the slightest impulse, and, therefore, the upper fragment cannot be disturbed in relation to the lower. If the body be inclined to the diagonal position, to right or left, this will not contort the upper fragment in relation to the lower, for the limb will instantly assume the corresponding position by the yielding of the cord, and the form of the limb will be preserved.

If the trunk descends towards the foot, the apparatus swings on the cord, and the upper fragment is not jammed upon the ower

3d. To those who have not witnessed the action of the apparatus, it

would appear difficult, by its use alone, to obviate angular deformity; but in this I have never found the slightest difficulty. The loops of the bandage on which the limb rests, exert a lateral concentric pressure, corresponding in degree to the weight of the limb, and tending continually to straighten it, and resist distortion. The traction exerted by the oblique cord conduces to the same result. A force thus exerted upon any angular and yielding substance will of course render it straight. Lateral concave splints may, if deemed necessary, be interposed between the bandage and the limb; but, although I have used them, and found them very easy of adaptation, I am satisfied that they are unnecessary, and especially because there are now no forces which tend to displace the fragments.

4th. The mechanism of the extension is sufficiently obvious. It is exerted by the obliquity of the cord, and is easily graduated by varying it. It is perfectly uniform and gentle, and does not require the tightening or relaxation of bands from day to day. It needs not to be strong, because the tendency to shortening is otherwise in a great degree obviated.

The force is exerted on the apparatus, and by it transmitted to the limb through the embrace of the whole extent of the bandage, so that there is no galling pressure from distinct extending bands. The starching of the strings and the loops of the bandage, the more completely obviates the slipping of the bandage. The splint, however, inclines to be a little drawn toward the foot, and, therefore, as stated above, the foot portion ought to be applied an inch remote from the instep, to prevent pressure on that point where it cannot be endured without injury.

It will be observed that, in the method of treatment here described, the necessity for severe mechanical constraint is superseded by evading the causes of displacement, rather than by contending with them. The limb does not become distorted at the place of fracture by the movements of the trunk, because the member thus suspended always maintains the same relative position to it; nor are the muscles provoked to spasmodic and unequal action, because there is no unequal and galling pressure.

This apparatus, with slight modifications, is applicable to all fractures of the femur. To none is it more appropriate, and in none has it accomplished more satisfactory results than in fractures of the cervix, the events of which are so justly regarded as an opprobrium of surgery. So uniformly has non-union and deformity resulted, that eminent surgeons have denied that bony continuity is ever restored within the capsule. We hope to show that these results are rather the consequences of insufficient treatment than defect in the reparative power of nature. This power is embarrassed by greater difficulties than in other fractures, and requires more efficient aid. This being furnished, union will often be accomplished.

Many surgeons, in fractures of the cervix, reject all mechanical means in the shape of splints, because so uniformly inefficient in promoting union, and because producing so much mechanical irritation. But if we can

furnish a support which shall sustain the member more comfortably than the softest pillows, and allow the trunk a little liberty in regard to position, it would be expedient to employ it, though bony union were not expected to be effected.

In fractures of the cervix there is required no particular modification in the application, as described above. The pelvic portion of the splint is here very important, and it should extend well upon the trunk, and should be firmly secured to the pelvis by the oblique and direct turns of the bandage. The falling outward of the knee, and the same contortion of the foot, with shortening of the limb, are the characteristic deformities, and are of course to be obviated. The apparatus being slightly angular at the knee, the bandage effectually prevents the falling out of that portion of the member, and the bandaging of the foot to the foot-piece effectually secures it in a natural posture.

The extending apparatus is here extremely important, and the obliquity of the cord must be carefully maintained.

In the treatment of no fracture is it so important to preserve a uniform posture of the limb in relation to the trunk, and to identify the motions of the former with those of the latter. We have already explained the mechanism of the apparatus in these particulars.

We shall present cases in which we have reason to believe bony union to have been accomplished. I would also direct the reader's attention to a case recorded by Prof. Geddings, of Charleston (see *The American Journal of the Medical Sciences* for Jan. 1847, p. 248), in which bony union was demonstrated as having been effected by the posterior apparatus introduced by me, and involving the same principles in regard to support and extension.

In the treatment of fractures through the base of the great trochanter (apt to be comminuted), the anterior splint is adequate to all the exigencies. Besides the general indications above described, the mechanism obviates a peculiar tendency to displacement incident to this fracture. The glutei muscles, acting as abductors, turn outward the upper fragment, and produce a salient angle externally. To obviate this tendency, the limb, as suspended, must be allowed to swing outward, by which the distorting muscles are relaxed, and their effect obviated. Indeed, the limb will spontaneously incline a little outward the moment it is raised, and the deformity will cease in consequence. It should be allowed thus to incline during the whole treatment, and any attempt to force it into a position parallel to the other member is mischievous, and the maintenance of this position is a serious objection to many forms of apparatus.

In fractures immediately below the lesser trochanter, the upper fragment, when the limb is allowed to fall into the horizontal position, is tilted forward by the action of the flexors of the femur—the psoas and iliacus muscles. The lower fragment, by the retraction of the muscles of the

femur, is drawn upward, and the limb shortened, as may be illustrated by these lines:—

Fig. 4.

To force the upper short fragment backward, into continuity with the lower, is impossible, by any force which can be endured. Here is a case, then, in which we must humour the muscles by posture, and not endeavour to coerce, by which we should defeat our own object. We must therefore elevate the lower fragment till we bring it into line with the upper, elongating the limb at the same time by traction, and thus adjusting the fracture.

This position is easily rendered permanent, and the coaptation made secure, by applying the anterior splint with the angles at the knee and hip more acute than represented in the cut, thus allowing a greater degree of flexion at the hip and at the knee; thus:—

Fig. 5.

The traction of the supporting cord will be exactly in the direction in which the upper fragment presents.

In fractures of the middle third of the femur I have observed, in almost all cases, the deformity to result from a salient angle of the fragments externally. I have examined many femurs of those who have been treated for this fracture, and almost always have found an external convexity of the thigh.

In these cases the abductor muscles, being inserted into the upper fragment, draw that portion of the bone outward, while the adductors, having their insertion continued to the lower extremity of the femur, draw the distal extremity of the lower fragment inward. When, in the use of any apparatus like Dessault's, the limb is approximated to, and made parallel with, the other member, the abductors are then rendered as tense as they can be, and the adductors placed in the opposite condition, and cause the drawing outward of the lower end of the upper fragment. Carry the whole limb outward, and the deformity disappears without the use of pressure.

To render the adjustment permanent, it is only necessary to suspend the limb in the apparatus—to allow it to swing outward, and thus to relax the abductor muscles. Indeed, the moment the limb is suspended it will instantly assume that position by the action of the muscles, they becoming relaxed by this movement, and ceasing to distort the fragment. Here is

another instance in which we succeed by humoring the muscles rather than by forcing the fragments into place by direct pressure. Coercion is certainly an impracticable principle in this department of surgery, provoking secession and the non-union of the fragments.

In fractures near the condyles, the upper extremity of the lower fragment is drawn backward into the ham by the heads of the gastrocnemius, especially if the leg be completely extended on the thigh. If we flex the leg, and, applying the hand to the upper part of the calf, make traction in the direction in which the femur points, the deformity disappears. We make this position permanent, and the adjustment secure, by suspending it in the anterior splint, making the angle at the knee more acute than that seen in the cut. The gentle support of the turns of the bandage in the ham contributes essentially to the desired result.

In fractures of the patella I have employed the same apparatus with signal success and comfort to the patient. The apparatus for this purpose must be made straight at the knee—the heel kept well elevated, so as to flex the thigh in some degree on the pelvis—the cord made perpendicular, or rather inclining toward the trunk of the patient, inasmuch as no necessity for traction exists. A compress must be placed above and below the patella, and the limb bandaged snugly up against the splint.

The application of the anterior splint to the treatment of fractures of the leg, especially compound and comminuted, has been not less satisfactory than in those of the femur. Indeed, the first case in which its usefulness was demonstrated was one of compound and comminuted fracture of the bones of the leg. The subject was Edward English, who came under my care in the Baltimore Infirmary, Sept. 28, 1854. He had been for months under treatment in good hands, in a neighbouring hospital. Amputation had been proposed, and would have been justified by the rules of conservative surgery. The patient, however, demurred, and prevailed on his friends to take him to the Baltimore Infirmary, with the hope that I would make further effort to save the member. I had every motive therefore to accomplish the object, if possible.

The leg was a shapeless mass of disease. The comminution, and lesion of the soft parts, had been very extensive. Many necrosed fragments were still included in the tumid parts; many fistulous openings existed along the whole length of the tibia, discharging matter from numerous sinuses. The limb was enormously tumid and of a livid hue from impeded circulation.

I employed the apparatus of wood, but extending no higher than six inches above the knee. It had the usual angle at the knee and at the ankle. Having placed it along the anterior surface of the leg, I first attached it loosely by adhesive strips, one passing under the foot, one under the ankle, one below the seat of fracture, one above it, and one above the knee. I placed a compress under the upper extremity of the splint where it rested on the front of the thigh. Staples were attached to the splint,

one near the knee and the other near the ankle, and by these the limb was suspended, the cord and pulley being employed as illustrated in the cut. I employed three bandages, two designed to be permanent and one movable. One was applied to the foot and carried up, with occasional reverses to effect accurate adaptation beneath, to the vicinity of the fracture. A second began above the fracture and was carried to the upper extremity of the splint, so as to grasp firmly the thigh. The region of the fracture was now exposed, but the support was complete. I now, after placing a basin beneath the limb and thoroughly washing it, applied the requisite dressings, and finally the movable bandage, embracing the region of fracture and many of the fistulous openings, arranging the turns so as not to obstruct the free issue of matter.

I was exceedingly gratified at the immediate results. The moment the member was slung the patient exulted in the relief which he experienced from the rigid confinement and constrained posture from which he had suffered while the limb lay an immovable mass, encumbered by splints, on the bed. He found himself capable of changing his posture at will, without the slightest hurt. The gentle concentric pressure of the bandage, determined by the weight of the limb, began at once to express the matter from the sinuses, and it was seen dripping copiously from the limb. The sinuses were thus emptied, and their walls compressed. The same mechanism promoted the absorption of lymph and serum from the tumid parts, and, together with the slightly elevated posture, aided the return of blood from the limb.

Improvement was immediate and rapid. The dressings were accomplished without the slightest disturbance of the limb, by removing the movable bandage and exposing completely the seat of injury. I removed, from time to time, considerable portions of necrosed bone, and laid open sinuses. The result was complete recovery with a useful limb.

The wire splint for the leg is applied as described above. In all fractures of this member above, or at, the middle, it is essential that the apparatus should extend above the knee, and grasp the lower third of the thigh. All the unavoidable movements of the thigh will then be communicated to the splint and will not disturb the upper fragment.

In fractures above the middle, the splint must be straight at the knee, so as to extend perfectly the leg on the thigh, and thus relax the muscles which would elevate the superior fragment, especially in oblique fractures. In compound fractures, the splint should not be wrapped with muslin opposite to the seat of injury. Adhesive strips should then be employed to attach the limb to the splint, before elevating it, because more cleanly. A permanent bandage should be applied below, and another above. A movable bandage encircles the wounded region. It may sometimes be expedient to interpose compresses at certain points between the shin and splint, to render the coaptation more perfect, but I have seldom found them necessary.

When the fracture is of the lower third, it is exceedingly important that the weight of the foot should be well supported, otherwise there will be a salient angle of deformity in front. This must be done without injurious pressure on the heel, and can be effected by supporting the sole of the foot well with the adhesive strap, which is drawn under the sole, and carrying the bandage with several well-adapted turns under the heel. The heel will then not suffer, because it rests on a surface perfectly adapted to its form. The turns may be starched after application, to render them permanent.

The weight of the limb, sinking into the apparatus, mainly exerts the force that restores and preserves the normal form of the member. The gentle traction of the cord, which should be but slight, contributes to the same result.

In compound dislocations of the ankle, complicated as they usually are with fracture, I have found the anterior splint far more satisfactory than any other means of support which I have employed. In those cases the foot must, as much as possible, repose upon its sole, where sustained by the adhesive strap. Another adhesive strap should be carried under the heel and over the splint. In all cases of injury at the ankle or near it, the lower hooks should be applied to the apparatus near the bend at the ankle.

When, in recent fractures of the leg, the member is first placed in the apparatus, the muscles will be, for a time, affected with a spasmodic rigidity and quivering which will for a brief time distort, in a degree, the fragments, and shorten the limb; but when it has reposed for a short time in the well-adapted concavity of the apparatus, the irritation ceases, and the fragments fall into their true relations. It is worse than useless to struggle with this tendency to distortion by direct mechanical pressure. If deformity continues, it is because the member is not well sustained by the proper adjustment of the supports.

If, in the application of the splint to fractures of the leg, the upper end of the apparatus is found to press too much upon the anterior surface of the member, the supporting cord must be slipped in the loop of the single cord, so as to shorten the portion to which the upper hook is attached. A compress should also be placed under the upper extremity of the splint. A pillow should be placed under the thigh, and the heel should be sufficiently elevated to clear the bed.

Care must be taken that the foot-piece does not press too much on the instep. It ought not to touch it when the apparatus is first applied. Often I interpose a thick, soft compress, and remove it in case the pressure becomes annoying.

It is generally not necessary, in fractures of the leg, to make extension. The cord, therefore, may be arranged perpendicularly. When it is necessary it will not usually be requisite to continue it in any considerable degree for more than four or five days.

It is worthy of remark that in the employment of this, or any other

apparatus which furnishes nearly perfect support to the fractured bone, and allows no appreciable motion, there will be no provisional tumour, or callus. The apparatus is the provisional expedient, and nature, instinctively aware of its existence, produces no exterior callus, but proceeds to restore the direct continuity. The time, therefore, occupied in restoring the rigidity of the limb will be greater, in some cases, than where less perfect support is given; but the ultimate direct union is sooner effected and far more perfect; nor will the muscles, after union, be embarrassed in their action by callus or by projecting fragments. The attentions necessary for the evacuations of the patient are greatly facilitated by this apparatus. There is nothing in the way obstructing the use of the bed-pan. When raised for this purpose the limb may be correspondingly elevated by slipping the block on the single cord.

NOTE.—It was my original design to have added to this article a series of cases showing the varieties of extensive compound and comminuted fractures to which "The Anterior Suspensory Apparatus" has been successfully applied by myself and others in this city and the surrounding counties; but by so doing its length would be extended beyond proper limits. I have employed it constantly since September, 1854, and some of the cases were briefly alluded to in a clinical lecture reported for the *Maryland and Virginia Medical Journal*, and printed in its 1st vol. for 1860. Since it was known that the above paper was being prepared many medical gentlemen have offered me illustrative cases, for which they will please accept my thanks.

ART. II.—*Sulphuric Ether and Chloroform as Anæsthetics, considered with reference to their relative Safety and Efficiency.* By FREDERICK D. LENTE, M. D., of Cold Spring, New York.

NOTWITHSTANDING the many plans that have been devised within the past few years for avoiding the dangers of chloroform inhalation, or for recovering the patient when threatened with its occasional alarming consequences, and the many ingenious contrivances, in the way of inhalers, invented for the same purpose; deaths, which, upon autopsy, can be attributed to no other cause than the anæsthetic, do still occur; and seemingly in as large a ratio as ever; for we are, every now and then, as we open a number of some foreign or domestic journal, greeted with the unwelcome caption, "Death from chloroform;" not occurring as they formerly did, in a considerable proportion of the cases, during the administration of the agent by dentists and non-medical persons, but in the practice of men well known in the profession. One effect of this, on the writer at least, is to produce a very unpleasant anxiety during the performance of any operation with the aid of anæsthesia, in addition to that which naturally attaches itself to

every operation of importance, and which, more or less, distracts the attention of the operator, according to the more or less reliance we place on the judgment of the person intrusted with the administration of the anæsthetic; and unless in a city, a reliable assistant is not always to be obtained. The important question is then still before the profession—*how are the dangers of anæsthesia to be avoided?*

Greater or less danger is inseparable from the administration of every powerful agent in the Materia Medica, however cautious and skilful the practitioner by whom it is employed; nor can we reasonably expect that an agent, so powerful as in a few minutes to render the body insensible to the pain of a torturing operation, shall be entirely exempt from risk. By what means, then, can we reduce this risk to its *minimum?* One tells us that it is to be effected by the use of his inhaler; another by his; another by some peculiar arrangement of the sponge or towel so as to insure a due admixture of atmospheric air, or by regulating the successive doses of the agent. But still the fact stares us in the face that the number of deaths is not diminished.

Various anæsthetics have been proposed, but only two have stood the test of time—sulphuric ether and chloroform. Both have been used to a great extent all over the world; but, for some years past, the latter has almost entirely superseded the former in Europe,[1] and still retains the confidence of many of the profession in this country. Five years ago, when the number of deaths from anæsthetics had fully aroused the profession to the necessity of seeking some means of diminishing its danger, the writer published an article in the *New York Journal of Medicine*, calling attention to the fact that it was from chloroform that all these accidents had occurred, and that the most obvious means of avoiding their repetition was its entire disuse, and the substitution of its less dangerous, but no less efficient competitor—sulphuric ether. Some months after this, an article, from the pen of M. Barrier, of the Hôtel Dieu de Lyons, appeared in a French journal, taking precisely the same ground, which was extensively noticed in foreign journals. But whether either article had any effect in directing the notice of surgeons more forcibly to the comparative safety of ether is not apparent. Since that date we have met with additional facts bearing on the relative value of these anæsthetics, and have thought that some good might perhaps result from again endeavouring to direct the consideration of surgeons to this matter.

In the first place, it is important to correct a very erroneous impression which has got abroad in consequence of the publication of certain European statistics of death from sulphuric ether and chloroform. These statistics have been recently referred to as authority in an editorial in the *American*

[1] Sulphuric ether is not, I believe, employed anywhere in Europe except in Lyons and Naples.

Medical Times (August 18th), which gave rise to a correspondence in the September number; from which it appears that they were taken from a work by Dr. Kidd, or from an extract from this work published by the author in the *London Med. Times and Gazette*. From these statistics it would appear that, out of one hundred and twenty-five deaths from anæsthetics in Europe, twenty-five are from ether, which, if accepted, would indicate that it is fully as dangerous as chloroform, the latter having doubtless been employed in at least five times as many instances. Upon examination, however, we find that the authority for the ether cases is, in most instances, a bare statement of M. Trousseau, without any reliable details whatever. Some of these alleged cases of death from ether are noticed by Dr. Snow in his work on *Anæsthetics;* and, except in two instances, the fatal result is clearly not chargeable to the ether, but to the shock of the operation; as for instance in the two cases on p. 365 (Lond. edit.). In one of these cases, the patient did not succumb until the third day; and in the other, forty hours after the operation, which was a very protracted one, and during which the patient was not even thoroughly etherized, thus failing, as it should have done, to diminish the shock. The coronor's verdict was, nevertheless, "Death from ether." I will only refer to two other cases (pp. 268–269). In one, the operation was an amputation of the thigh for compound fracture of the thigh with extensive laceration, and a simple fracture of the other thigh. The etherization was not perfect during the most painful steps of the operation, and its shock thus undiminished; as might have been anticipated from the nature of the injury, the patient died, but not until *three hours* after the amputation. In the second case, a cancerous tumour, weighing three and a quarter pounds, was removed from the breast of a feeble old man; and here also the fatal result occurred only after the lapse of *seven hours*. Dr. Kidd's language is too vague to admit of any confidence in his reports regarding so important a matter as the one under discussion. He speaks of deaths from ether, and from a mixture of ether and chloroform, and from amylene, in such a confused manner that one is completely in the dark as to how many are to be attributed to each of these agents individually, even if it be admitted that they were due to the anæsthetic, and not to the accident or operation. But if any one wishes to get a clear idea of how utterly unreliable Dr. Kidd's writings are, he would do well to peruse the review of his "little work on chloroform" in the *London Med. Times and Gazette*, or the *Edinburgh*, or the *Dublin Medical Journal* for 1858. Whether the death in any given case is to be attributed to the anæsthetic, or the shock, or hemorrhage, or other accident connected with the operation itself, is often a matter of opinion or judgment, and sometimes, perhaps, even of prejudice; as in some of the cases above alluded to. Therefore, each case, to be accepted as authentic, should be carefully given in detail, and, if possible, accompanied with the autopsy. For instance, in two cases of alleged death from sul-

phuric ether recorded in Dr. Snow's work (pp. 362–64), the author repudiates the idea of the death being due to the anæsthetic; whereas, both cases certainly admit of doubt, and most readers would find the ether guilty in one instance.[1]

Let us now inquire how many authentic cases of death from sulphuric ether are to be found on record. Happily they are few indeed. The only European case worthy of credit, and this repudiated, as we have seen by the highest authority, is that which happened at the Hôtel Dieu d'Auxerre. At the autopsy, the spleen was found disorganized; but the general condition of the patient was good previous to the operation, which was for cancer. The case which happened under M. Barrier's hands at Lyons, I think, we may fairly attribute, with Dr. Snow, to the hemorrhage and shock of the operation. With regard to American cases, I will first notice that which happened at Bellevue Hospital in the city of New York; which will be found well reported in the *New York Journal of Medicine* for July, 1859. It is hardly fair to attribute the fatal result in this instance to the anæsthetic, when it is known that the autopsy revealed a *large malignant tumour of the cerebellum;* also that the patient had been suffering for weeks with unmistakable symptoms of serious disease of the brain; unsteadiness of gait, loss of vision in one eye, partial loss of mental power, constant tendency to roll out of bed, always to one side; and especially when it is known that sudden death is very apt to occur, if indeed, it does not always occur, in these cases of tumour of the cerebellum. Dr. A. Clark referred to two cases at the meeting of the Pathological Society of New York, at which this case was reported, of sudden death, where the autopsy revealed tumour of the cerebellum. Dr. Eve relates two cases of what he considers death from inhalation of sulph. ether, in the *Southern Med. and Surg. Journ.* for 1849, republished in the *Am. Journ. of Med. Sci.* for the same year. The first was that of a medical student, who, for amusement, inhaled about an ounce of the liquid, and became furiously excited, so that it required several persons to restrain him. He fell into a deep sleep, was awakened, and became again excited and uncontrollable; then was allowed to be quiet, and slept well all night, complaining next morning only of a slight headache. This soon increased to severity, and he died of symptoms of *phrenitis.* But no autopsy was held, nor is there any evidence of inquiry into his previous history, as to symptoms of cerebral disease. It is at least doubtful, admitting phrenitis to have been the immediate cause of death, whether this was due to the ether directly, or to the violent excitement caused by the etherization having been arrested previous to complete insensibility. Dr. Eve's second case occurred in the

[1] Dr. G. Hayward, of Boston, who is excellent authority in this matter, agrees with Dr. Snow, that no well attested case of death from sulphuric ether is recorded. (British and For. Med.-Chirurg. Rev. for 1859.)

practice of his friend Dr. Bassett, at Huntsville, Ala. The patient was labouring under a violent attack of tetanus; and, we may infer from the history, that he was well nigh exhausted by the disease before the consultation was held at which it was determined to give ether. The report states that, *in one minute*, he was under its full influence, and in a quarter more, he was dead. This case occurred shortly after the discovery of the anæsthetic influence of ether; and probably none of those present were acquainted with its phenomena. If they had had the experience of the present day, they never could have arrived unanimously at the conclusion that the patient died of the effects of the ether. It is almost, if not absolutely impossible to induce full anæsthesia with ether in the time alleged; the supposed anæsthesia was doubtless the death-stroke of the disease.[1]

But it scarcely falls within the scope of this paper to discuss the use of anesthetics or their dangers in *medical* practice; and we might, with propriety, have omitted all reference to these cases, were it not that we might be suspected of concealing unfavourable facts, in order to place sulph. ether on a more favourable basis. If anæsthetics be made use of in medical practice, especially in those violent, dangerous, and oftentimes almost incurable cases to which they are generally applied, as in tetanus, epilepsy, puerperal convulsions, delirium tremens, cerebral diseases, and the like, we must expect a sudden death now and then—either from the disease, and coinciding with the administration of the anæsthetic, or from the combined effect of the disease and the remedy—just as we occasionally witness the death of a patient from croup, during the attempt to save life by tracheotomy. I have myself desisted from administering chloroform to a man labouring under tetanus, who had been relieved again and again by it of his terrible suffering, because it was evident that he was liable to die during the application of the remedy. I have abandoned anæsthetics in the paroxysms of delirium tremens, because I found there was danger of immediate death from the combined effect of the vapour and the violent resistance to its reception by the raving patient. I deem it my duty to allude here to two other *surgical* cases which I know to have happened recently, where the ether might be considered as chargeable with the fatal result. In one, at least, from the history of the case, I think it was so. It is to be regretted that the history of these cases cannot be fully given. In one the patient was exceedingly reduced, and in an almost hopeless condition, previous to the operation; but the death, which occurred suddenly, and during the insensibility from the anæsthetic, was probably not entirely due to the shock of the operative procedure. In the second case, the operation, which was not a "cutting" one, necessitated a very unfavourable position for giving ether, the head being flexed forcibly on the chest; and it was while

[1] Dr. Hayward relates one case, which he saw at Naples, where anæsthesia was effected with sulph. ether in one minute and a third. But this case is unique.

in this position, that the patient was noticed to be in a dying condition—too little attention having probably been given to the state of the respiration and pulse. Both these operations were performed by a skilful and experienced surgeon.

We thus admit *three deaths* from sulph. ether throughout the world. It is safe to say that there have been at least a hundred from chloroform in Europe alone. It is impossible to give the *ratio* of deaths to the *number* of inhalations of these two agents respectively, which is very desirable. But, we know that, for one year before the introduction of chloroform into practice, sulph. ether was the sole anæsthetic; that it was very extensively employed in Europe and America; that ever since, ether has been almost the only anæsthetic in some of our large cities, and for some years past almost exclusively used in most of our large hospitals in the United States. Dr. Geo. Hayward, of Boston, who was the first surgeon to use an anæsthetic successfully, in a capital operation, and to whom is due the credit of having made the most persevering and successful efforts to obviate the dangers of anæsthesia, by recommending the disuse of chloroform, had personal experience of near a thousand cases of ether inhalation, near two years ago, which of course only comprise a certain proportion of the cases that have occurred in his city under other surgeons. Dr. Hayward informs us, in a letter just received, "that ether is administered in this city (Boston) daily to a great extent, without producing death in a single instance, or any alarming or troublesome symptoms." In the New York Hospital, ether has been almost, if not entirely, the only anæsthetic for some twelve or fourteen years. It has been stated, by French authority (*L'Union Médicale*), that from 18,000 or 19,000 chloroform inhalations in the Crimea, only two deaths took place—one in the English, and one in the French army. The assertion of M. Baudens, that among 30,000 cases in the Crimea, no death occurred from anesthesia, is denied by other French army surgeons, who themselves saw deaths from it. As we have before asserted, it is a matter of individual judgment, in many cases, as to whether the death is to be attributed to the anæsthetic; and we may well suppose that, on the field of battle, it would be extremely difficult, if not impossible, in most cases, to judge whether a death, occurring during a terrible operation or soon after it, while the patient is still suffering from the shock, is due to the one cause or the other. This must have been especially the case on the sanguinary fields of the Crimea, where the surgeons were so overtasked that they could have had but little time or inclination to note the relative effects of the chloroform and the knife, or the injury. In the hospital operations the case might be different; but even there, the same cool judgment cannot be bestowed on the cases as in civil hospitals, where medical assistants and spectators are numerous, and not personally absorbed in the operative procedure. If these reflections are unjust, it is unaccountable that such a disproportion of deaths to the number of cases of inhala-

tion should have occurred in military surgery, and under comparatively unfavourable circumstances. Every medical man who reads the periodicals of the present day, especially the foreign, must be struck with the frequency with which "death from chloroform" meets the eye, and yet it would require many months to add up 30,000 cases of chloroform inhalation in civil hospitals; and it is from hospitals almost exclusively that we get our reports of fatal cases. The ratio of fatal to successful cases of chloroform inhalation must therefore be greater than these military statistics would indicate.[1]

But, there are other evidences in favour of the superior safety of sulph. ether besides those deduced from statistics, which latter may be considered, and are to some extent, imperfect, as regards both these agents. First, it is admitted, universally, that ether is less powerful than chloroform, and therefore there is less danger of that sudden and unexpected action, which has been the cause of most of the deaths from anæsthesia; or, of the ill consequences of carelessness or inattention on the part of the person intrusted with its administration, which has no doubt been the true cause of not a few of the fatalities.

The evidence of physiologists, who have largely used both these agents in quieting animals subjected to vivisection, points strongly to the greater safety of ether inhalation, the occurrence of accidental death being very common under the use of chloroform, and very rare with ether—the insensibility being complete in both instances. My friend, Dr. H. B. Sands, of the University of New York, found it so in his numerous experiments, and he informs me that Prof. Dalton, the distinguished physiologist of the same institution, has met with like results. Dr. Snow, whose authority has been so often quoted, and whose opinions on these subjects are regarded with the greatest respect by the profession in Great Britain, says, p. 362, "I have not been able to kill an animal in that manner (meaning the sudden manner in which he found chloroform act) with ether, even when I have made it boil, and administered the vapour almost pure." He adds, "I hold it therefore almost impossible that a death from this agent can occur in the hands of a medical man, who is applying it with ordinary intelligence and attention." The following incident, which recently fell under my own observation, is sufficiently interesting in this connection to merit an introduction here, for it seems to settle the question, as far, at least, as the inferior orders are concerned, of the relative safety of the two anæsthetics under discussion. My old friend, Mr. John Lyell, a teacher in this place, has a large number of bees, with whose operations he has long

[1] It is a significant fact, alluded to by Dr. Snow, that although ether was used over a year in Europe and America before the discovery of chloroform, only one death occurred from anæsthesia. Whereas, chloroform was only introduced to the profession in November, and in January a death from it occurred, and soon after, others in various parts of the world.

been in the habit of amusing himself, during his leisure hours, and wishing to take his honey without destroying his pets, asked my advice about resorting to anæsthetics. I advised chloroform, as being the stronger and more convenient agent, and the one usually employed for such a purpose. He accordingly tried it; but, on finding that he lost a great many bees by suffocation, he substituted sulph. ether. This he found equally efficacious, and, what is very remarkable, that he did not by it lose a single bee, while he showed me more than a quart that had been destroyed by the chloroform, the insensibility having been just as perfect and prolonged in both instances, and both agents employed in the same manner, and under the same circumstances. It may not be out of place to relate a case which occurred a year ago, in my own practice, as showing the greater facility with which recovery ensues from too thorough anæsthesia from ether than from chloroform. The operation consisted in the removal of a fibrous tumour from the submaxillary region, together with the submaxillary gland. It was requisite that the insensibility should be complete; and, in addition to this, the necessity for keeping the handle of a knife in the mouth in order to force out the tumour, and render it salient externally, tended to impede the respiration still more. The ether was superintended by a surgeon attached to a large hospital, and well acquainted with the action of anæsthetics, who noticed, after the inhalation had proceeded satisfactorily for some time, and after the operation was nearly finished, that the respiration and pulse were both failing. Further proceedings were immediately suspended, and for a time the danger seemed imminent. The window was opened, a little water dashed in the face, and the patient allowed to remain perfectly quiet. In a few moments he revived, the respiration and pulse resumed their natural rhythm; he was again brought under the influence of the ether, and the operation completed. It was not only my opinion, but that of at least two of the hospital surgeons present, that had it been chloroform instead of ether, the case would not have terminated so favourably.

Another case, illustrating still more forcibly this matter, occurred a short time since at the New York Eye Infirmary, to which I am permitted to allude, by Dr. Noyes, of that institution. The patient had been thoroughly *etherized*, and had been breathing stertorously, when his countenance was observed to be livid, his respiration to cease, and his pulse very feeble. He was turned on his side, his tongue drawn forward; and, very soon, without any further treatment, his pulse and respiration resumed their wonted fulness and regularity. Some cases have been reported of apparent death from *chloroform*, in which the patients were resuscitated; but it was only by the most energetic means, including skilful and protracted artificial respiration. But, it is unfortunately true that artificial respiration, which, by Marshall Hall's method, is so successful in cases of asphyxia from quite protracted submersion, and in suspended fœtal animation, and even after

poisoning by narcotics, has very generally failed in suspended animation from chloroform inhalation. A few cases, however, of resuscitation of patients asphyxiated by chloroform vapour, which have fallen under my notice, seem to indicate that artificial respiration, if resorted to at once, and before loss of time, and possibly additional injury to the patient by other less efficient measures, might prove less unsuccessful. Prof. Metcalfe, in a paper on this subject read before the New York Academy of Medicine, some years ago, mentions four cases of recovery after pulse and respiration had ceased. One occurred in his own practice. He promptly applied his lips to the mouth of the patient, and inflated the lungs, while another physician compressed the thorax and produced expiration. Prof. Valentine Mott informs me that he had two cases in his own practice, where the patients were rescued from impending death by prompt artificial respiration. One was the case of the wife of a medical gentleman in New York; the other was a child, on whom he operated for stone. I find a case in a late number of the London *Lancet*, of which the following is a brief abstract: Chloroform was administered to a girl 23 years of age, for the purpose of amputating the thigh. Everything seemed to be going on as usual, and the operation was about to be commenced, when "she *suddenly ceased to breathe, and the pulse could not be felt;* cold water was instantly thrown on her face, air freely admitted into the room, and artificial respiration kept up by alternately compressing and relaxing the chest." No success. "The head fell on the chest, the chin dropped—in fact, she appeared quite dead. As a last resource, in order to use artificial respiration more effectually, it was decided to open the trachea, and inflate the lungs through the wound. This was at first done with the mouth applied to the wound, with some success. A female catheter was now introduced into the trachea, and artificial respiration kept up through it." This was fully successful. In most, I believe in all the above cases, the respiration was kept up by the old method—*insufflation;* in the last case, we see that it was successful after the mechanical method had failed. It may be that chloroform asphyxia is not as amenable to the "ready method," and other similar means of inducing respiration, as the asphyxia referable to other causes. It is certain that to be effective it must be far more prompt.

It is proper to make a brief allusion to a mixture of chloroform and sulphuric ether, which has been recommended as an anæsthetic, with a view to combine the advantages of both, and to lessen the danger of the former. It was at one time used to a considerable extent. A death from this anæsthetic occurred in the service of M. Valette, of Lyons; and another is reported from Virginia; but, from the history of this case, there is doubt whether the fatal result might not have been, with greater propriety, attributed to the operation. My own opinion of this mixture, without any actual experience, is that it is more dangerous than either the one or the other anæsthetic alone. For, the person intrusted with its

administration, supposing it to be less dangerous than chloroform, gives it more rapidly, and perhaps with less care; whereas, he may be giving the chloroform almost as pure as if it had not been mixed; for the ether being far more volatile than the chloroform, six times, according to Dr. Snow, is soon dissipated, and the latter left on the sponge to be inhaled alone. Unless they could be uniformly evaporated together, I see only danger in combining them.

But, even if admitting the greater safety of sulph. ether, its opponents argue that, after all, the proportion of deaths from chloroform is extremely small compared with the aggregate number of cases of inhalation; that no powerful drug in the *materia medica* can show as clean a record, and that the complexity of the apparatus necessary for giving it, the inconvenience, the delay, the quantity required, and other alleged disadvantages of ether, more than counterbalance its greater safety. Let us now examine these objections; but, first, I am ready to admit the wonderful safety of chloroform inhalation, considering its astonishing power in annulling pain and spasm, and that the proportion of fatal cases to the actual number in which it has been employed under almost every conceivable circumstance, is extremely small. But, if we have a simple means of reducing this proportion still more, as I contend we have, it is our duty to do so. The objections above enumerated are urged by those who have not given ether a fair trial, and would soon vanish if they were deprived of chloroform. I know this from actual experience. Connected with the New York Hospital, at the period of the discovery of the anæsthetic power of sulph. ether, as assistant to the house surgeon, it was one of my duties to administer it to patients about to undergo operations, and no difficulty was experienced in producing its full effect. It was administered by all the surgeons until the introduction of chloroform. It was now soon discovered that ether was troublesome, that large quantities were required, increasing the expense, and that it often failed to produce full insensibility even after considerable delay; it would often be cast aside after a few minutes' trial, and chloroform substituted; with a few drachms, or perhaps an ounce or so of this, and the delay of only a few minutes, the patient would be in a stertorous condition, and all trouble over. The inference was plain, ether was uncertain in its operation, productive of unnecessary delay, and ought to give place to chloroform; and so it did. But, soon one or two deaths from it were reported; and, one day, while we were chloroforming a patient, to whom I had myself assisted in giving it but a short time before without a bad symptom, he suddenly died, and nobody was to blame on the coroner's inquest, and to nothing else could the unfortunate result be attributed but the anæsthetic.[1] So to avoid a similar occurrence, as no one had then heard of a death from ether, it was resolved to return to it; and, after

[1] Case reported in Dr. Snow's work, p. 136.

this, we had no more difficulty in etherizing patients. But let us take up the objections in detail, and consider them fairly. First, as to *quantity*, being a less powerful agent than chloroform, it must of course be used in larger quantity to produce the same effect, but by no means in the enormous quantities alleged by those who prefer chloroform; that is, if properly administered; and the proper mode of giving it will best be considered here. If one accustomed to the careful and gradual manner in which chloroform must needs be given to insure any degree of safety, attempts to give ether in a similar manner, and without bearing in mind that it is not liable to that sudden action of the former, and especially that it is six times more volatile, he will certainly fail, or be subjected to great delay and inconvenience, and use enough of the drug to put half a dozen patients asleep. I have known skilful physicians to do this very thing, and have put patients sound asleep for the dentist with two or three ounces, who had been previously subjected to the fumes of ether for half an hour, in one instance, the patient said, an hour, involving also the waste of near a pound of the anæsthetic, the effort finally ending in failure. For an adult, two or three ounces, sometimes less, are abundant for the production of full anæsthesia for a painful operation. For children, from three drachms to an ounce and a half, according to age; children are affected much more readily and pleasantly by anæsthetics than adults, with the exception of the preliminary struggles from fright, during which the inhaler should be kept closely applied. By using a properly constructed inhaler, even less ether would probably be consumed. And this I conceive to be the only advantage of an "inhaler." As regards delay, I think it is increased; as regards safety, I cannot see that it is enhanced by any of the various apparatus invented. In the very first fatal case reported as having occurred from ether, an "inhaler" was used.[1] The principal alleged advantage of a regular inhaler is the regulation and due admixture of the air admitted. But this can be just as well regulated by a proper arrangement of the sponge, or even the towel, as will presently be explained. My friend, Dr. Squibb, of Brooklyn, has recently, through some mistake, represented me as advocating the inhaler which he highly approves of, made by rolling up a thick, folded towel in a cylindrical form, open at top and bottom; the ether is poured around the inside of this, and the open top insures a safe amount of air—unfortunately, for its success, too large an amount. On this recommendation, Dr. Weir, of the New York Hospital, tried it, but found that it caused much waste, both of time and ether. I will quote from an article on this subject, furnished by me to the *New York Journal of Medicine*,[2] to explain my mode of employing it, which is the plan

[1] From Scotland we have had fewer reports of death from chloroform than from any other part of Europe, although this anæsthetic is exclusively used there, and still on a folded handkerchief or towel, as at first.

[2] September, 1855.

originally adopted in Boston, and now generally used throughout the United States, with some trifling modifications. "To be effective within a reasonably short time, it must be given rapidly, and the *access of air cut off as much as possible*, the reverse of what is safe with chloroform. The only effective inhaler we have used or seen used, is a large cup-shaped sponge, sufficient to cover completely the nose and mouth, and covered with a thick folded napkin, to prevent undue evaporation as far as possible." I now pour into the sponge about half an ounce of ether, and, covering it fully with two thick towels placed together and folded into a square large enough to cover both it and the face to some extent, approach it gradually, but without much delay, close to the face of the patient, after directing him to take full inspirations through nose and mouth. If he is disposed to hold his breath during the inhalation, or to cough, I withdraw it a trifle to allow a larger admixture of air, and then immediately approach it again to the face; and as soon as it can be inhaled fully, without coughing or strangling, the sponge is kept lightly *in contact* with the face, and the towels held completely over the sponge and face, gathered in around the edges so as to cut off *evaporation of ether*, and *ingress of air*. The half ounce is soon followed by about an ounce, which is rapidly poured *into*, and not *on* the sponge, though the latter is occasionally preferable, as recommended by Dr. Hayward. The necessity for replenishing being judged of by the time, or by the nose applied to the edge of the sponge a little raised. When insensibility is *complete*, judged of by the relaxation of the muscles of the arms, or by touching the *conjunctiva oculi*, the sponge is either entirely or partially removed from the face, according, sometimes, to the nature of the operation, sometimes to the character of the respiration and pulse—stertorous breathing contraindicating even the smallest quantity of the vapour.[1]

As regards the *quantity* of ether required in protracted operations, it is not so great as is generally supposed. I have no statistics to guide me; but, more than a pint, if judiciously used, even in the most tedious cases, is seldom necessary—the patient gets so saturated with the ether, after six or eight ounces have been inhaled, that very little, applied from time to time, will keep up full insensibility. The last protracted operation of mine, a few nights ago, required the patient, a female in delicate health, to be constantly kept in a state of full anæsthesia for at least an hour and a half, and not more than eleven ounces of ether were consumed, and it was administered most of the time by an apothecary. Dr. Hayward says from four to eight ounces, but he means for the whole operation, not merely for the preliminary anæsthesia, of which we have been speaking, when alluding

[1] I have never heard that this precaution, of cutting off as far as possible, all admixture of air, has been attended to anywhere, either here or in Europe, except in Naples, where, Dr. Hayward informs us, it is very successful.

to the minimum quantity; and he adds, that less would be required if care be taken to exclude atmospheric air, which he either thinks not quite safe, or unnecessary. I am well aware that much larger quantities of ether are used than are spoken of as sufficient by Dr. Hayward and myself, even where this agent is extensively employed, as for instance, even now, at the New York Hospital. But, still, if they proceeded upon the principle of saving ether, and of using only enough to produce the desired effect, even without entirely excluding the air, as here recommended, much less would be consumed. But, it is obviously idle to discuss quantity, except with reference to time; for, what difference does it make, in a painful and important operation, whether the surgeon uses four ounces or eight ounces? If quantity is referred to by those who object to ether, with reference to expense, a pound of Dr. Squibb's article, the best for inhalation in the market, in our judgment, can be purchased for half a dollar, and two fluid-ounces of his chloroform costs as much or more.

As to *time:* to induce full anesthesia in an adult with sulph. ether should not require more than five minutes, sometimes six. With children much less time is required. Dr. Snow gives two or three minutes as the average for children, and four or five for adults. And how much sooner can a patient be *safely* chloroformed? Dr. Hayward states that "*in no case* were more than eight minutes required to produce complete anæsthesia." This is the maximum; he does not state the average.

I do not desire to be understood as ignoring all danger and all necessity for caution in the administration of sulph. ether. On the contrary, I hold that every surgeon, who performs an operation with the aid of any anæsthetic, is bound, if possible, to secure the services of some competent person to attend to this department, or to look to the condition of the patient himself from time to time; and this he may generally do in the case of ether, an agent so uniform and regular in its action, without distracting his mind too much from the operative procedure itself. It is of great importance also to attend to the state of the pulse for a short time subsequent to the completion of the operation; for, occasionally, alarming prostration has followed anæsthesia, after trivial operations, even where the inhalation has proceeded with perfect regularity and success. I do not of course allude to those alleged *deaths* from ether, previously referred to in this paper, where the patients died hours, and even days after protracted and dangerous operations, but to such cases as one which I reported in the *New York Journal of Medicine*, for 1856, and which is referred to in the last edition of the *U. S. Dispensatory*. I have recently met with one almost precisely similar, of which, the fear of occupying too much space in the Journal, forbids any notice at present. Should vomiting succeed the administration of the anæsthetic, the patient being partially under its influence, the air-passages may become suddenly clogged by the lumps of food which some people bolt, and thus endanger life. We have not always

the opportunity of enjoining an empty stomach, which is advisable. Another danger, that we occasionally meet with, is the falling backward of the tongue, as in the case at the Eye Infirmary. There is no danger, however, that may not be readily avoided by a little care.

An important preliminary with patients who are nervous, or alarmed at the idea of taking the ether, is to get their confidence, to assure them positively of the thorough safety of the drug, and to explain the sensations which they are likely to experience in passing through the several stages to complete anæsthesia; especially the choking feeling which immediately precedes total insensibility, and which often otherwise causes a struggle, and delays the operation. It is sometimes necessary to administer the agent in an adjoining room, when there are a number of spectators or assistants assembled. With sulph. ether, it is also sometimes advisable to close the doors and windows, if open, should there be a current of air, as I have found this to delay the anæsthesia. The objection urged against ether, that its odour is disagreeable, and saturates the clothes, and penetrates the whole house, is too puerile to merit notice. Its inflammability is a more valid objection during operations by candlelight; but, a little care will remove all danger from this source.

I am aware that all this minute account of a simple matter, which everybody is supposed already to understand fully, will be considered by many only tedious and useless; but, that it is not entirely unnecessary, is apparent from the following paragraph, which I quote from an editorial in a leading London Journal.[1] "The complexity of the apparatus necessary for its administration, the large quantity required, and the time taken up, are the great hindrances to its more general use in this country." It has been my humble endeavour and sole object in writing this paper, as it was in coming before the profession previously on the same subject, to aid in correcting these very erroneous impressions regarding sulph. ether, under which many of the profession in this country, and almost the entire profession in Europe, are labouring.

NOTE.—Soon after forwarding the above paper to the Editor, I observed, in a medical periodical, a brief notice of a case of "death from sulphuric ether," said to have been read before the Cincinnati Academy of Medicine, by Dr. W. H. Mussey. Upon application to the reporter, I was very promptly furnished with the details of the very interesting case, which will be found in the *Cincinnati Lancet and Observer*, January, 1861. From a careful consideration of these details, I cannot attribute the fatal result to the anæsthetic. Only *four* ounces of ether were used; and, as it was poured on a folded towel, and from twenty to twenty-five minutes, apparently, consumed in its administration, there having been two intermissions on account of vomiting, but a very small proportion of these four ounces must have been breathed. And when the operation was *commenced*, the administration had been discontinued on account of some peculiarity of the breathing, "the pa-

[1] London Medical Times and Gazette, July, 1858.

tient screaming out and writhing with pain, and apparently perfectly conscious." Prompt artificial respiration, and other energetic means, failed to restore life.

The autopsy revealed extensive injuries (from the accident), sufficient, in the opinion of Dr. Mussey, and several other physicians present, to have caused death in a short time. The brain was not examined; and the left ventricle of the heart was empty, which is not the case after death from suffocation, or from anæsthetics.

Dr. M. remarks, that—"A lengthy discussion arose in the academy, occupying two evenings of its session, in which, two members contended that the case was clearly one of death from ether. Of the remaining disputants, two thought ether possibly auxiliary, while a majority thought ether not at all responsible."

ART. III.—*On Perineal Section.* By J. MASON WARREN, M. D., one of the Surgeons to the Massachusetts General Hospital.

THE distinguished surgeon, Mr. Syme, of Edinboro', has of late years brought more particularly to the notice of the profession the incision of the urethra in the perineum for the purpose of relieving strictures of a bad character. Some confusion seems to exist as to the cases for which this operation is more particularly applicable. So far as we can understand Mr. Syme, his operation appears to be directed to those cases which are permeable to the instrument, yet so irritable and intractable as to be a permanent source of irritation to the patient. Mr. Syme considers that every stricture, with patience, is capable of being penetrated. Other gentlemen, and particularly Mr. Bryant, in an excellent paper in Guy's Hospital Reports, think, there are certain cases of stricture admitted to exist by most surgeons which no patience will overcome, and which the use of the knife alone will remedy. In these he advises, that an instrument should be introduced into the urethra as far as allowed by the obstruction; the finger of the left hand is then introduced into the rectum as a guide to the prostate or neck of the bladder. The integuments now being divided, the knife is introduced with its back towards the rectum so as to strike the urethra in front of the prostate; and a cut is made forwards till it meets the instrument in the urethra.

Mr. Bryant, after mentioning the different forms of stricture requiring this operation, rejects that in which with retention of urine the urethra is impermeable to an instrument; for which case he advises as the only proper operation the puncture of the bladder through the rectum. Without entering upon a discussion as to the merits of the operation, we would simply state, that in our experience this is the very case which most frequently calls for the active interference of the surgeon; and there seems to be no good reason, if the history indicate a callous and incurable stricture as the cause of the retention, why the operation should not be so extended as to

divide the stricture at the same time, and give the patient a chance of permanent instead of temporary relief.

In order to illustrate this subject and bring it more fully before the profession I propose, by the exhibition of two or three cases, to show the advantage of the operation in these extreme cases which we are so often obliged to deal with; as it is well known that in permeable strictures, to which Mr. Syme's operation would be applicable, the patient, so long as he could obtain relief otherwise, would be unlikely to submit to a cutting operation. One of these is a case in point with those mentioned by Mr. Bryant, viz., a case of organic stricture of long standing gradually diminishing the calibre of the urethra, and finally terminating in complete retention. In this case, it will be perceived, that instead of puncturing the rectum, as would have been the practice of Mr. B., the urethra was cut down upon in the perineum, opened near the neck of the bladder, and then the whole portion of the strictured urethra divided; and a large instrument being introduced the urethra was moulded upon it until a complete cure was produced. The other two cases are those in which the operation of Mr. Syme must have been rejected; as, after much patient treatment in one of them no instrument could be made to enter the bladder, and in the other, although for a time an instrument could pass through the stricture, ultimately this became impracticable, and the patient's life was gradually being exhausted by pain, confinement, and the irritation and discharge from the numerous fistulous openings in the scrotum and perineum. To Mr. Syme undoubtedly belongs the credit of having fully brought this subject before the profession, and shown the great relief that might be extended to a class of cases allowed to linger on in torment through many years.

In his very valuable paper Mr. Bryant has indicated yet other cases, a class to which the attention of hospital surgeons is much more likely to be called, and of which the preceding are an illustration. We take the liberty of copying Mr. Bryant's conclusions, to which we fully agree, with the exception of the first, upon which we have commented above.

"*Conclusions.*—1. In uncomplicated retention of urine from organic stricture, the operation of opening the urethra in the perineum is not required, the more simple and safe one of puncturing the bladder through the rectum being preferable.

"2. When complicated with extravasation of urine from any cause it should be performed at once, and the stricture, when present, divided, if possible.

"3. In laceration of the urethra from injury, when a catheter cannot be passed, the urethra should be opened.

"4. And also when the above injury is associated with pelvic mischief.

"5. Strictures are occasionally met with which are impermeable, and urethras which are obliterated.

"6. That in cases of organic stricture, when the passage of a catheter is possible and not difficult; where it does not produce either any injurious, or painful constitutional or local disturbance, and where, after dilatation of the stricture, an occasional passage only of the instrument is required to maintain an open channel, no other surgical means can be called for.

"7. That cases of stricture do occur occasionally, which are so exquisitely

sensitive, and in which the passage of a catheter, however skilfully performed, is followed by such severe constitutional and local disturbance as to produce more harm than good; and others, which are relieved by means of a catheter, and are even fully dilated, but which have a tendency to contract again immediately upon the omission of the treatment; in such cases the operation of 'external division' is most valuable.

"8. That the majority of cases of what are called impermeable strictures may be rendered permeable by constitutional treatment, but that some are undoubtedly impermeable; in such cases the operation of 'perineal section' is of value.

"9. When the urethra is obliterated the operation of 'perineal section' may occasionally be demanded, particularly when associated with perineal fistulæ.

"10. That the worst and most intractable forms of stricture are the result of injury, and in those cases the operation either of 'external division' or 'perineal section' is of great value.

"11. That in boys the operation is not so successful as in adults, although no better can be suggested."

CASE I. *Organic Stricture of the Urethra of Ten Years' Standing; the Urine finally passed in Drops; Retention; Perineal Section; Division of the Stricture; Complete Recovery.*—A. L. Foydt, about thirty years of age, entered the hospital on April 30, 1860, with a stricture of the urethra of ten years' duration. The water, when he entered, passed in a very small stream, and urine was constantly dribbling away from him during the night, accompanied with a purulent discharge. He complained of pain in the renal region, though not of a severe character. After very careful attempts to pass a bougie of the smallest description, it was finally decided to attempt to cut the stricture from within, which was done from a canula with a concealed blade, on May 15, and which, although it cut but partially through the stricture, seemed to afford him temporary relief.

On May 26 the urine became less free; there was considerable dulness in the pubic region, and the patient complained of pain about the bladder, with general uneasiness. I had already made up my mind to perform the perineal section in this case, and ordered the preparations for the operation to be made for the following day.

On visiting him the next day, I found he had a complete retention of urine, and therefore proceeded at once to the operation. The patient was placed on a table, on his back, and after being thoroughly etherized, so that his joints were fully relaxed, he was tied as in the operation for lithotomy. I have found it much better to confine the limbs in this way than to intrust them to the care of assistants, who are apt to be worn out during an operation so likely to be long and tedious. A small staff was introduced, and it seemed to penetrate the first stricture, which, as stated above, had been incised about a couple of weeks before, and brought up against a second stricture, apparently just behind the root of the scrotum. The forefinger of the left hand was now introduced into the rectum, and the situation of the prostate ascertained. The perineum was then divided, and a careful dissection made, to ascertain the site of the urethra. This was rendered very tedious from the constant discharge of blood at the bottom of the deep wound from the bulb of the urethra, which in this case was more than ordinarily troublesome. The urethra, however, was finally opened directly in front of the prostate, and a gum-elastic catheter passed into the bladder, giving exit to a very large collection of urine. The canal was now opened forwards, and the callosities freely divided, until the staff was reached. A second gum-elastic catheter was now passed downwards through the penis until it appeared in the wound; and the ivory end of the first having been

cut off, the point of the second was insinuated into it, and firmly fixed. In this manner it was dragged up through the whole extent of the urethra. It might be here mentioned, as a hint for finding the urethra, which I have before practised upon, without the staff for a guide, and where other means have failed, to do as was done in the present case—viz., allow the patient partially to recover from the ether, stimulate him to make an effort to urinate, and when the urethra behind the stricture became dilated, a minute stream of urine issued, indicating the spot for the introduction of a probe, and the urethra was found. No unpleasant symptoms followed the operation. On the next day the patient expressed himself in a state of great comfort, more so than he had experienced for many years. The sense of fulness, arising from probable distension of the bladder, ureters, and pelves of the kidneys, having entirely passed away. At the end of a week the first catheter was removed, and at once replaced by another, which had been prepared the day before, of the same size and curve. The catheter was found to have been partially acted upon by the urine, and its calibre somewhat obstructed.

At the end of a month the patient was able to introduce the instrument himself; and at the end of two months, the wound in the perineum having nearly healed, he kept it in at night, leaving it out during the day.

He shortly after left the hospital, well, with the exception of a small aperture not larger than a pinhole, and some weeks subsequently presented himself there on the eve of a voyage to sea.

CASE II. *Stricture of the Urethra of Twenty-five Years' Duration; numerous Urinary Fistulæ in the Scrotum and Perineum; Perineal Section; Cure.*—E. O. B., shipmaster, entered the Massachusetts General Hospital on April 24, 1860, and gave the following history of himself. He had suffered from gonorrhœa twenty-five years previously, and was treated by his captain at sea by strong injections. The gonorrhœa terminated in gleet, which was treated by bougies for two years, when it was renewed on account of increased obstruction. He then remained pretty well until eight years since, when the strictures became so tight as to give him a great deal of inconvenience, for which he entered the hospital, under my care, and the stricture was divided by internal incision. This relieved the urinary trouble, and for a time he enjoyed good health. Two years since a fistulous opening appeared in the perineum; four months later another in the scrotum; five months since a third; through these openings purulent matter with urine was freely discharged. The whole of the scrotum was tense, indurated, and barred by sinuses. Two months previous to his admission a No. 1 bougie was passed into the bladder, after much effort; his symptoms have been aggravated by it.

I passed a No. 1 bougie down to the stricture, and kept it applied for two or three hours daily, exerting a gentle pressure against the stricture. After treatment for about a week in this way, the stricture gave way, and the instrument passed into the bladder.

On May 1, I made an incision into one of the scrotal openings, and gave vent to a quantity of purulent matter mixed with urine. Notwithstanding the passage of the bougie, the symptoms of trouble about the scrotum increased, and towards the 1st of June, the stricture having again closed, so that it was quite impossible to pass any instrument, the operation was done, at his request, on June 4.

The patient being etherized, and confined in the position for lithotomy, Syme's sound was passed through one stricture, and encountered a second.

An incision was now made in the median line of the perineum, and dissection performed until the point of the staff was reached. The tissues were much hardened, and infiltrated by a deposit, which greatly opposed the progress of the knife. The flow of blood from these diseased parts interfered much with the operation; the perineum also was uncommonly deep. In order the better to bring the continuation of the urethra beyond the stricture into view, the end of the staff was turned out through the incision, and served to hook up and thus bring the deeper parts more fully in sight. The wound being freely sponged with iced water, a puncture was made into the supposed urethra, in the neighbourhood of the neck of the bladder, which allowed the passage of a probe into that organ; and the urine having escaped, showing it had gone in the right direction, a large gum-elastic catheter was substituted for the probe. A sharp-pointed knife was now passed up by the side of a small Syme's sound, and the first stricture, which lay behind the middle of the scrotum, and which would not allow anything larger than the sound to pass, freely divided.

A second gum-elastic catheter was now introduced through the glans and urethra, insinuated into the mouth of the first, which remained in the bladder, and was thus drawn upwards as in the former case.

No great constitutional irritation followed the operation, and the patient on the following day described the comfort of freeing the bladder by a large stream, after having suffered for so many years in his efforts to do so, as beyond belief. The first bougie was left in place a week, then becoming obstructed, it was replaced by another, and this was done weekly through the course of the treatment. The wound in the perineum, on account of the diseased state of the tissues, was very slow in healing, and the patient remained in the hospital for two months afterwards.

A communication was received from him in Nov. 1860, in which he states that he was well.

CASE III. *Traumatic Stricture of the Urethra of Five Years' Duration.*—G. U., 36 years of age, a teamster, came into the hospital under my care in the early part of the spring of 1860. He said that five years before he had been jammed against a wall by the buffer of a freight car with such force as to produce a rupture of the urethra. He remained in a critical situation for a time, and had never since been able to pass his water, except in a very small stream. Two years previous to his admission, a fistula appeared near the tuber ischii of the left side, between that and the rectum. His water has passed by drops through this fistula, and another in the rectum, ever since. Various attempts have been made to perforate the stricture, but all of them without avail.

On examination by the rectum I found that the bladder, intestine, and surrounding parts were glued together and involved in an indurated mass, and the calibre of the intestine was very much diminished. A probe being introduced into the fistula by the side of the rectum, passed up by the side of the gut through these indurated tissues, and apparently entered the interior of the bladder. Having made very careful attempts for a time to get through the stricture by gentle means, I finally advised him, unless willing to have the perineal section performed, to desist from any further efforts, for fear of producing irritation and complete retention, especially as he did not suffer much from his disease.

In June, having heard of the success of two of the above operations, he came back to the hospital for the purpose of having one performed upon himself.

Operation.—A sound was passed down as far as the stricture, an incision made upon it, and its point reached deep in the perineum. No traces of the urethra beyond could be discovered by the most careful manipulation. He was, therefore, allowed to recover partially from the effects of the ether, and to make an effort to void his urine. An incision was now made in the direction of the neck of the bladder, and a female catheter introduced as a guide, which was replaced by a No. 8 elastic catheter. The parts were very vascular, and the steps of the operation, which lasted over an hour, much obscured by blood.

Not the slightest unfavourable symptom followed; but at the end of the week, when it was necessary to replace the catheter, it was found to be a matter of much difficulty to do so, the end of the instrument escaping into the rectum. This was, however, finally effected both at that time, and afterwards, by hooking the beak of the instrument against the pubes, and then, instead of trying to force it forwards, the handle was suddenly depressed, and it slipped into the bladder. Carried forwards in the ordinary way, it always went into the rectum.

On the 27th, it was recorded that all the urine was discharged by the catheter, although there was purulent discharge through the fistulous opening near the rectum, and occasionally from the rectum itself. The patient remained in the hospital rather more than two months, when he left, and has since been seen well.

ART. IV.—*On Fever and Inflammation.* By J. L. TEED, M. D., of Mendota, Ill.

NEXT to the various processes of healthy nutrition, fever and inflammation present the greatest interest both to the physiologist and pathologist—they enter singly or conjoined, into nearly every condition of disease; primarily or by their results, they lay by far the larger portion of civilized humanity in its grave; they meet us at every turn, and present themselves under nearly every phase: it is no wonder, therefore, that from the earliest days they have engrossed, and still continue to engross so much of the attention of physicians.

Yet these processes are even still mysteries. After all that has been said and written about them, and the innumerable disputations, how little has been reduced to the certainty of science, and what a vast portion of our fancied knowledge still remains hypothesis or conjecture!

Whence the cause? Partly the very difficulty of the subject, and its intimate connection with the processes of life, themselves so little comprehended; and partly because physicians, while yet ignorant of the natural workings of the healthy body, having observed a few more superficial facts of its diseased conditions, have immediately framed hypotheses by which to explain them; when each observer meeting some variances in the facts, and finding some discrepancies in the hypothetical explanations of the others,

has thought it necessary to rebuild the whole fabric from the foundation; thus hypothesis has been piled on hypothesis, until the student in sheer bewilderment sides with one or with another, without knowing exactly why, and then declares his party the true faith, and brands all others as heterodox or heretic.

These conditions have many things common to both, as well as peculiar to each. Their very names exhibit one of their connecting circumstances—increased heat; in inflammation this is local, in fever general; the part *inflammat*, the whole *fervet;* they arise also from a common cause, therefore they are seldom met with separately, for inflammation is scarcely ever present without fever, and fever, unless periodic or febricula, is seldom observed free from all inflammation; and indeed several pathologists have declared fever to be an inflammation, some of one organ, some of another. Either one, however, may be the primary or the secondary disease. They differ principally in that inflammation is essentially a local disturbance, while fever is a general derangement—inflammation, while consisting of a nervous as well as a vascular element, is yet more particularly seated in the organic tissue itself, starting from which as a centre, it may implicate the whole frame—so as to have been declared a perverted nutrition; while fever has no local habitation, no fixed point of departure, but from the very commencement seems to be a general disorder of the whole organic functions. In inflammation the peculiar product is an excess of fibrin, in addition to the varied results arising from the affection of different organs; in fever the tendency is to a diminution of the fibrin and of the red corpuscles, the results of a general derangement of the secreting functions being added.

Pathologists have endeavoured to condense these processes into that *multum in parvo* a definition; but from their complex nature, this attempt has been generally a failure. Galen described fever as *calor præter naturam*, and Virchow writes, "fever consists essentially in elevation of temperature, which must arise from increased consumption of tissue, and appears to have its immediate cause in alterations of the nervous system." Copland had previously written, "that the morbid impression (producing fever) is first made upon the organic or ganglial nervous system, and owing to the circumstances of this system actuating the circulating, secreting, and excreting viscera, is manifested in an especial manner by the changes observed in the state of vascular action, in the animal temperature, in the functions of secretion and excretion, in the circulating fluids, and in the other functions which are more or less intimately dependent upon the ganglial system." Virchow adds, "Every disease may become febrile, every disturbance may form itself into a fever if it extend itself to the centres which regulate the waste of the tissues, and the proper moderating power of the tissue metamorphosis is suspended."

Dr. Southwood Smith observes, "that the order in which the morbid actions, which constitute fever, occur, is first, derangement in the nervous and sensorial functions, this is the invariable antecedent; secondly, derangement in the circulating function; this is the invariable sequent; and thirdly, derangement in the secreting and excreting functions; this is the last result in the succession of morbid changes. Derangement in the functions of secretion and excretion never comes first in the series; derangement in the nervous and sensorial functions never comes last in the series; derangement in the function of circulation never comes either first or last in the series, but always the second in succession."

Inflammation has been defined as increased sensibility and vascularity of a part, with a tendency to morbid secretion, and consequent disorganization. Dr. Bennett defines it as an exudation of the normal liquor sanguinis, and Dr. Alison as a *tendency* to exudation.

In the number of this journal for July, 1860, I traced the histological arrangement of two of the tissues, the muscular and the osseous, as typical of the whole, to be that of cells in various approximations having one surface in connection with a nutrient vessel of the plasmatic vascular system; both vessel and cell being filled with distinct fluids, which pass from one to the other according to their osmotic conditions; and this interchange of fluids constitutes nutrition. If, therefore, liquor sanguinis osmoses from the vessel it must be into the cell, for the wall of a cell forms a part of the limiting membrane of the vessel. One of two things may happen, the fluid may osmose too largely *into* the cells, causing over-distension of them; or it may remain in the vessels and distend *them;* and in addition to either of these, it may rupture the walls of some vessels and cells, and blend the whole into one confused mass: either of these states, except the first, can be caused only by some sudden impediment to the circulation in vessels nearer the heart, as may arise from sudden contraction of the vessels at the seat of the disorder. Should the engorgement come on gradually, a larger amount of plasma may circulate through the part, and exude largely, or osmose into the cells, but becoming organized, hypertrophy will be the result, or some other structural change, and yet be unattended by inflammation. So also an impediment may gradually arise to the circulation in a part, causing obstruction in, and osmosis from the plasmatic vessels, distending the cells, or perhaps even rupturing some vessel and cell-walls without it becoming organized, and yet be unattended by inflammation, as in some dropsical conditions. It seems to be only that local condition, which is attended by an increased formation of fibrin in the fluids, a loss of the contractile power of its smallest vessels with dilation and engorgement, and an irritated state of its contained nerves which should be considered as inflammation.

The nerve, the vessel, the ultimate tissue element, and the fluids in the part are all unitedly concerned in the production of inflammation, nor can

it exist without the implication of them all; no two even of those factors can produce it.

This same histological arrangement, being applied to the term waste of tissue, naturally raises the question, what is the tissue which suffers this increased consumption in fever? If we examine closely the phenomena of nutrition, it can be only the albuminous pabulum of the blood, the similar portions of the cell fluids, the blood-corpuscles, and the adipose deposits in the body, for we have no evidence that the cells, whether muscular, nervous, osseous, or other, are broken up and carried away, but the contrary, unless the forming of fresh Haversian systems in bone be considered such a removal of cells, although it may admit of other explanations. If this increased consumption were a result of a natural or healthy cause, it might be replaced by an increase in the food, as happens in increased exertion, &c. &c.; in fever the increased consumption is accompanied by a diminution of the appetite, and with impairment of the assimilating functions. But while there is an increased consumption, there is no increase in the amount of fibrin contained in the blood, unless some local inflammation be conjoined with it.

We may thus see the important differences between these two processes arising from their elementary composition, and extending through all their relations, treatment, &c. &c. Increased heat is a symptom of both diseases, but its production, as much as that of fibrin, remains an unsettled question; it is chiefly the result of chemical decompositions and oxidations; while the friction of the blood against the vessels may assist in its formation, as also may nervous currents, as is the case with ordinary electricity—however any increase in the quantity of blood circulating through a part will elevate its temperature, and an acceleration of the general circulation is more frequently attended with the same result, if not invariably so—the local increase in the circulation attends always in inflammation, the general as constantly in fever—and to moderate this activity of the circulating system has been a constant desideratum in the treatment of these diseases, whether by venesection, arterial compression, digitalis, the revived veratrum viride, or indirectly by promoting the evacuations. Inflammation as well as fever may arise from causes originating with the body, as well as from those affecting it from without—while particular causes and particular differences in the individual will occasion corresponding differences in the fever or inflammation produced, and thus may arise sporadic cases of those fevers generally contracted by contagion or infection; for in such diseases the *first* case must have originated *suâ sponte;* but in all accelerated circulation and increased heat are constant factors.

Starting from the acknowledged basis, that fever is the result of an effect produced on the nervous system, two methods of treatment have been promulgated; the first is to act directly on the nervous system, and restore its equilibrium; the second is to remove the effete productions until the

cause has ceased to operate, or the incited action has expended itself—the first, attempts to cut the fever short; the second, denies this possibility—in the first secernents, in the second, stimulants are used as adjuvants; of course the main stay of the treatment being the reverse. And notwithstanding the intrinsic differences between them, the same theory and plan of treatment have been adopted by either party in regard to inflammation.

There are many objections to the general use of the means with which we are at present acquainted, that exert a powerful influence on the nervous system; they do not act on all alike, and they are not always controllable in their action—instance the different effects of morphia, strychnia, digitalis, prussic acid, &c., on different individuals.

The second plan limits the formation of effete matter, counteracts its influence while it remains in the system and constantly promotes its removal.

The first method is exemplified in the cure of periodic fever, where a powerful stimulant taken shortly before the accession of a paroxysm, puts a complete stop to the disease. The second plan, indeed, from its very character of expectancy, cannot present such a striking example, but by comparing the general results of the two modes of practice in continued fevers and inflammations, they are greatly in favour of the second.

This free use of stimulants is based on the theory that disease arises always from deficiency of nervous force, either direct or indirect; it ignores the fact so strongly, and so continually dwelt on by Copland, that vital manifestations, or the results of the vital forces, may be vitiated in kind as well as in degree; it looks only at the force acting, instead of also considering the matter acted upon; and forgets that with increased tissue change, is an increased amount of effete products, which by their very presence vitiating the blood stream, produce an unhealthy condition of parts previously unaffected, while the very stimulants poured in increasing the products of decomposition, the nervous centres are reacted upon from their peripheries, organs already overworked, or unable to work from congestion receive fresh excitation, and additional local and general disorder are awakened, under which the disease and the patient are terminated together.

One argument raised in favour of alcoholic stimulants in continued fevers and inflammations, is their alleged power of reducing the action of the heart. But the action of the heart is subject to great variations in these diseases spontaneously, it will vary on different days, and even on different parts of the same day: it is, however, difficult to conceive how a stimulant, which as its ordinary results causes acceleration of the heart's action, and increased fulness in the pulse, will reduce the action of the heart while the pulse remains full, and it is not until the pulse has become *soft* as well as *frequent*, as in the latter stages of continued fever and inflammation, that the use of alcoholic stimulants has been found most serviceable.

The blood plasma circulating around every cell in the body, as well as being the vehicle of nutrient matter to them, is also the channel by which

effete matter is removed from them. In a state of health this nutrient matter is derived from a due manufacture of alimentary substances into the albuminous pabulum of the liquor sanguinis; while the effete matter is removed by the secreting and excreting organs. Paralysis of the organic nerves supplying these glandular structures, is followed by a cessation, or at least by a diminution of their secreting function. The blood stream may pass through them in increased quantity, but the secretion is hindered, and its peculiar constituents retained in the blood. Should this be the case with only one organ, then by the principle of compensation, other organs act vicariously; when, however, the disturbance becomes general, and the secretions of all are hindered, the trouble is increased in such a degree, that life is soon terminated unless they are restored—under the second plan of treatment mentioned above, the object is, from the earliest stages, to promote the action of those organs by which effete matter is expelled, and by abstinence or low diet reduce their work as much as possible. The excreta being capable of division into two classes, the nitrogenous, and the non-nitrogenous, and the chief of the former being urea and uric acid, those remedies which have the most effect in promoting their secretion are of especial value in the treatment of both diseases, especially in their earlier stages, and liquor ammoniæ acetatis, and bicarbonate of potash, or its citrate, have descended to us as heir-looms of great value, although from the researches of Dr. Garrod, lithia and its carbonate appear to possess superior powers to them.

The non-nitrogenous excreta are derived largely from the intestinal canal; but owing to the great tendency to diarrhœa in many of these disorders, and the consequent debility thereby induced, and owing to the fact that the small quantity of food allowable may require longer time than ordinary for its digestion and assimilation, great care must be always observed in the use of purgatives, of which enemata may be in a great majority of instances the better kind; while it is essential that they act, it is also essential that they do not act too much. In both fever and inflammation the action of the skin may be advantageously promoted. Tepid sponging with alkaline solutions, or with diluted alcohol, will be found very effectual, and contribute much to the ease and comfort of the patient. The pulvis antimonialis comp. of the Pharmacopœia, or James' powder as usually called, is by far the best diaphoretic for general use, and when combined with small doses, one-sixth or one-eighth of a grain of acetate of morphia, a little camphor, and nitrate potash when allowable, will often lull a febrile paroxysm after a few repetitions, and induce a copious diaphoresis; a calm and quiet sleep; a moist tongue, and a general improvement with a gentle increase of all the secretions.

Another remedy much used in both these diseases, and in nearly all their stages and kinds, is mercury in one or other of its forms, and no remedy has been so abused by a reckless use, so much so as to have become a by-

word and a reproach to the profession. It is usually given in what are termed alterative doses. This term alterative is exceedingly vague; it means everything or nothing, and justifies the saying of Chateaubriand "that words were invented to conceal our thoughts," and our ignorance, it may well be added. An alterative is a remedy which changes the character of the secretions without increasing or diminishing them. But we neither see this effect produced, nor do we expect to see it in the use of mercury in either fever or inflammation. If quicksilver or any of its preparations is digested with solution of chloride of sodium, decomposition ensues, chloride of mercury, *i. e.*, corrosive sublimate is formed, and soda from oxidation of the sodium by atmospheric oxygen, and this process is facilitated by the addition of a little blood; chloride of mercury is soluble in excess of albumen; and the double compounds, chloride of mercury and albumen, with chloride of sodium or with chloride of potassium, present considerable resistance against decomposition by oxygen. The mercury, therefore, passing into the various cells from the blood stream in these double compounds, as it were, withdraws a large amount of the blood on the one side, and of the tissues on the other, from the action of oxygen as effectually as venesection, while the volume of the blood not being altered, the action of the heart is lessened, having to circulate that amount of unstimulating matter; and the blood itself is at the same time just so much diluted, absorbs so much less oxygen, and is consequently so much less stimulating. There is no necessity in the majority of cases to cause as much mercury to be absorbed as the system will tolerate without disturbance; for it is not until this point is reached that the particular local effects of salivation are produced, and its remedial effects are obtained previously; its approach may be easily recognized, except where the system is very impatient of this mineral; and when we wish to rid the system of it, a rapidly oxidizing agent, as the chlorate of potash, causes its decomposition and discharge.

During the formative stage, or the stage of incubation, any zymosis spreads rapidly through the whole system; while all the excretions being checked, the opportunities for its discharge are more and more lessened; in the inflammatory stage the nitrogenous excreta are increased, and remain above their natural standard as long as this stage lasts, while the chlorides are as constantly diminished, the patient discharging a large excess of nitrogen, over and above that consumed in his food; the carbon and hydrogen excreted not being so easily estimated, but they manifest greatly disordered conditions as evinced by the fetor of the breath, of the stools, and of the sweat. In this stage the circulation becomes accelerated, and the pulse full and bounding, in proportion to the sthenic character of the disease. The connection between this condition of the pulse and the intensity of the disease is so obvious, and the remedy so instantaneous, that bloodletting has been the practice from time immemorial; and in incipient inflammations of a sthenic type under the diminished blood stream, the

pulse will often become soft and compressible, all the secretions will begin to flow, and the patient pass directly into the stage of convalescence. It is only when the case has been of long standing, or the cause of a specific character, or the disease too closely approximating the asthenic or typhoid condition from the first, or too apt to run into it, that bloodletting is inadmissible; but in all inflammations where the vascular element is more involved than the nervous bloodletting—while in the opposite, that is, where the nervous is more involved than the vascular—counter-irritation should be resorted to, and often with considerable freedom. In inflammation of a distant joint, arterial compression in the limb above has been often followed by good effects, the diminution of the blood stream being in this case confined to the part affected.

These diseases form an excellent confirmation of the correctnesss of Liebig's division of food into plastic and respiratory. We observe, and more particularly so in fever, how largely the nitrogenous portions of the body are removed, as evinced by the large excess of nitrogen discharged over and above that contained in the food; and also by the great amount of caloric set free in the system from the secondary decompositions thereby occasioned; while the large quantity of cold water, and often of ice, consumed is but barely sufficient to render the internal heat bearable. The food in these cases is always extremely scanty, for nature has wisely abolished the desire for it, and weak, thin broths, or whey, are generally preferred, and drinks acidulated by some vegetable acid. The carbo-hydrates and fats, more especially the latter, are extremely ill-suited to this condition of things, and if taken are very apt to produce intestinal disturbances. It has been lately objected to Liebig's view, that fat, salts, and water are as essentially components of the tissues as albumen. This objection is very superficial, for all nitrogenous food contains both fat and salt, and the animal substance void of them, gelatin and its allied substances are not, although rich in nitrogen, capable of being substituted for them. It is not nitrogen that makes *plastic food*, that is not Liebig's point; but that food to be plastic must contain nitrogen. This by no means denies that a body may contain nitrogen, and yet be unfit for food; it only affirms, what no one has yet proved to be false, that animal life cannot be supported on non-nitrogenous food; but may be supported solely on single articles of food, as milk, flesh, bread, and that these articles, besides other things, are very rich in nitrogen; and not only are fever and inflammation in their relations to food, good examples of the truth of Liebig's views, but the more fully these views are understood and carried out by the physician in the treatment of these diseases, and of the periods of convalescence from them, the better will it be for his patients, and the less likely will they be to suffer relapses.

In examining fever or inflammation under general principles, great care should be taken not to select peculiar forms of fever or inflammations of particular organs as types of these diseases in general, by which to guide

our treatment of them as a class. No two forms of fever, and inflammations of no two organs, are to be treated on any stereotyped plan; and it may almost be added, no two cases of either disease. Peculiarities of the individual, and of the circumstances are almost infinite, and will cause variations of treatment correspondingly numerous—and the laying down rules for the use of stimulants, or purgatives, or any other remedy, or the disuse of bloodletting as a constant practice in inflammation generally from a special practice, in inflammation of a particular organ, or in fever, from a particular epidemic, will tend rather to reduce the practice of medicine to a shallow empiricism, than to extend its rational and scientific foundations, and plant the minds of its students and practitioners on those vito-chemical principles which deal with the body as a material reality, and its changes as real material changes; and when these are unknown will lead them, not to invent hypotheses, but to institute investigations to obtain the facts. The present state of physiology has been attained chiefly on the foundation of animal chemistry; for although the microscope has revealed the minute structure, we have not been able to gain thereby a definite idea of the function of the part; but by learning more and more fully the chemical characters of each secretion, and by carrying the investigation into the laboratory, we have obtained information which has yielded valuable results. The same plan in pathological physiology will add greatly to them, and when observers shall take the vito-chemical stand-point, medicine will progress as a science and as an art.

ART. V.—*On the Action of Potash, Soda, Lithia, Lead, Opium, and Colchicum on the Urine.* By WILLIAM MOSS, M. D., of Philadelphia.

THE following experiments were made with the design of determining the influence of the above-mentioned substances over the quantity of the urine, and the amount of its solid, organic, and inorganic constituents, with especial reference to the amount of urea and uric acid. They were all performed upon myself during the winter months of 1860–61.

My age is 27 years; height 5 feet 5½ inches; weight 125 pounds; health good. I regret that it was impossible for me to regulate accurately the quantity and proportions of my food, and the amount of exercise, further than to make them, in a general way, as uniform as possible.[1] This disadvantage necessarily attends experiments extending over several months, and is common to most of the previous experimenters on this subject. The quantity of urine was determined in cubic centimetres, and the weight of

[1] I take two meals daily: breakfasting, *à la fourchette*, at 9 A. M., drinking at this meal strong black tea; and dining at 5 P. M.

the solids in grammes. The method employed for the determination of the amount of urea, was the volumetric analysis of Liebig. The amount of uric acid was determined by quantitative analysis, as the accuracy of the volumetric analysis of Dr. Scholtz is not as yet sufficiently authenticated. To determine the whole amount of solid matter, I subjected ten cubic centimetres of urine to a gentle heat, in a large air-bath, during forty-eight to seventy-two hours, and completed the evaporation, in vacuo, over sulphuric acid. The residue was accurately weighed, and the whole amount found by simple proportion. By heating the residue, mixed with nitric acid, until the carbon was consumed, and subtracting its weight from the whole weight of the solids, I ascertained the respective weights of the inorganic and organic solids.[1] The specific gravity was determined with a specific-gravity bottle weighed in a delicate balance.

The medicines were all ingested, with the exception to be mentioned hereafter, at one hour before breakfast, one hour before dinner, and before retiring for the night; *i. e.*, at 8 A. M., 4 P. M., and 12 P. M. The urine was collected from 8 A. M. to 8 A. M.

Normal Urine.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Nov. 13, 1860	1100	50.72	38.17	12.55	28.93	.284	1019.60
" 14, "	752	44.10	34.83	9.20	29.98	.257	1019.00
" 15, "	948	61.83	48.66	13.16	36.97	.306	1026.00
" 16, "	838	58.24	46.57	11.67	33.97	.382	1036.00
" 19, "	757	53.01	44.10	8.90	27.98	.319	1027.00
" 20, "	689	54.58	46.47	8.11	33.09	.430	1028.00
" 21, "	703	48.32	39.08	9.24	25.87	.371	1027.00
" 22, "	611	41.33	33.61	7.71	22.47	.310	1028.00
" 23, "	812	50.15	38.65	11.50	30.48	.455	1026.90
Average	801	51.36	41.12	10.22	29.97	.346	1028.61

Acetate of Potassa.—Of this salt, I took an ounce per diem, dissolved in an ounce of water, and divided into three equal doses. During its administration, I carefully abstained from increasing the daily amount of food and water ingested. The urine passed was of a pale straw colour and strong alkaline reaction.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Nov. 24, 1860	1400	73.18	45.69	27.47	32.609	.317	1023.8
" 26, "	1305	74.59	50.87	23.65	34.256	.564	1025.6
" 28, "	897	55.13	35.67	19.46	26.469	.231	1026.4
Dec. 1, "	1198	65.44	45.15	20.28	35.997	.404	1025.3
Average	1200	67.085	44.345	22.715	32.332	.379	1025.27

[1] There is probably some slight excess in the weights of the solids and the organic constituents, as it was impossible to expel the whole of the water by this process.

Acetate of Soda.—An ounce of this salt was taken daily, in the same manner as the preceding one. The urine passed was of a pale straw colour and alkaline reaction.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 3, 1860	981	58.90	40.68	18.21	32.856	.082	1028.9
" 4, "	1000	55.44	42.49	12.95	28.821	.103	1025.5
" 5, "	1010	54.27	34.58	19.68	26.351	.297	1025.8
" 6, "	1090	60.05	36.61	23.43	29.633	.199	1027.1
Average	1020	57.165	38.59	18.56	29.415	.170	1026.82

Acetate of Lead.—The ingestion of this salt lasted four days, during which I took nine, eight, nine, and nine grains, respectively, per diem. It induced thirst, constipation, and finally slight griping. The urine passed was acid and high coloured. It was tested daily, with sulphuretted hydrogen, for lead—which first appeared in the urine of the third day. On the fourth day, also, it was present. On the fifth day, on which I took no lead, repeated examinations failed to show its presence. On the sixth and seventh days, I took five grains of iodide of potassium per diem, and obtained faint traces of the lead; after which, it did not reappear.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 8, 1860	800	52.43	41.50	10.93	28.704	.398	1024.4
" 10, "	797	49.43	39.22	10.20	25.410	.398	1024.0
" 11, "	721	48.16	38.37	9.79	27.104	.349	1025.9
" 12, "	773	51.86	37.44	14.41	30.332	.039	1028.0
Average	772	50.47	39.13	11.33	27.887	.296	1025.57

Carbonate of Lithia.—Fifteen grains of this salt were taken daily; grs. v, water f℥j. The urine was of a pale colour and faint acid reaction.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 20, 1860	1146	60.03	44.16	15.86	30.526	.431	1021.0
" 21, "	1500	73.83	50.90	22.92	36.908	.457	1019.6
" 22, "	1151	55.41	44.09	11.32	31.223	.409	1018.0
" 24, "	1253	56.02	43.31	12.71	29.999	.186	1016.4
Average	1262	61.32	45.61	15.70	32.164	.370	1018.75

Opium.—During this series, I took for two days one and a half grains of opium per diem, in pill form, in three doses; and, for the two following days, two grains daily, in four doses. Urine high coloured, reaction strongly acid. The dryness and thirst from which I suffered while under the influence of this drug, were so great that I was unable to refrain from largely increasing my ingestion of water.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 27, 1860	1139	58.09	46.41	11.61	27.98	.104	1020.0
" 28, "	1300	57.00	48.24	8.75	29.62	.277	1015.3
" 29, "	1209	52.22	39.48	12.74	25.16	.046	1016.8
" 31, "	1052	51.41	37.67	13.73	27.23	.340	1026.3
Average	1175	54.68	42.95	11.70	27.50	.191	1018.35

Colchicum.—Sixty drops of the wine of the fresh root were taken daily. Urine of strong acid reaction.

Date.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Jan. 6, 1861	899	57.94	44.82	13.11	33.817	.384	1027.0
" 7, "	806	54.50	43.67	10.82	31.027	.222	1027.3
" 9, "	1029	54.67	41.61	13.01	27.872	.204	1020.8
" 10, "	900	54.50	40.51	13.98	28.908	.507	1024.2
" 11, "	816	53.31	40.56	12.74	27.151	.331	1026.9
Average	890	54.98	42.23	12.73	29.755	.329	1025.24

Table of Averages.

	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Average of Normal urine	801	51.36	41.12	10.22	29.97	.346	1028.61
Acetate of potassa	1200	67.08	44.34	22.71	32.33	.379	1025.27
Acetate of soda	1020	57.16	38.59	18.56	29.41	.170	1026.82
Acetate of lead	772	50.47	39.13	11.33	27.88	.296	1025.57
Carbonate of lithia	1262	61.32	45.61	15.70	32.16	.370	1018.75
Opium	1175	54.68	42.95	11.70	27.50	.191	1018.35
Colchicum	890	54.98	42.23	12.73	29.75	.329	1025.24

It will be seen, on reference to the above table, that the amount of normal urine secreted by me was far below the usual average, which is from 1400 to 1600 cubic centimetres. During this first series of investigations, the weather was cold, and, with the exception of one rainy and one cloudy day, remarkably clear and dry, with a thermometrical range of from 42° to 55° F.; so that the small amount of water cannot be due to increased action of the transpiratory function. I subsequently repeated my measurements with the same results, the average for three days being 824 cubic centimetres. The normal averages for the solids, urea, and uric acid, are below the medium found by previous observers, but not disproportionately to my weight and size. The high rate of the specific gravity was caused by its unaccountable, and, I may add, vexatious, rise, on the fourth day of the series, to 1036; a number so clearly abnormal that I consider myself justified in eliminating it from the series—the average of which without it would be 1025.18.

The experiments with soda, potassa, and lithia, were conducted with especial reference to the relative influence of these alkalies over the elimination of uric acid from the system in the gouty diathesis. Under the use of potassa, the excretion of urine was increased by one-half, with a notable augmentation in the amount of all its different solid constituents. The soda increased the amount of water, while the amount of organic solids was decreased, and that of uric acid was diminished one-half. The augmentation in the amount of inorganic matter is attributable to the egestion of the alkali itself.

The results of the experiments with lithia are confirmatory of the high praise awarded by Dr. Garrod[1] to the action of this agent. Although taken in infinitely smaller quantity than the potassa, the effects produced by it, with the exception that it failed to render the urine alkaline,[2] were almost precisely similar. The amount of organic matter excreted during its ingestion was even greater than during that of the potassa, while the difference in their average weights of uric acid was less than a centigramme. Dr. Garrod, I believe, first called attention to the fact that, during the ingestion of lead into the system, the excretion of uric acid is diminished. I also found this to be the case, though to no very considerable extent; but, as it will be seen on reference to the table that the amounts of water, organic matter, and urea were likewise diminished—the inorganic matter alone undergoing no diminution—it is a fair inference that lead exercises no specific influence over the uric acid, and that the decrease is but incidental to the general diminution of organic activity caused by this metal.

The value of the results of the experiments with opium was doubtless affected by the large quantity of water ingested during its administration, and to which we may attribute the increase in the amount of water and solids excreted. Of these last, however, the amounts of urea and uric acid fell—the latter by nearly one-half.

The result of the experiment with colchicum agrees with those obtained by Drs. Krahmer and Garrod—viz., that colchicum is not a diuretic—and differs from that of Dr. Hammond; for, although there was an increase in the quantity of urine and of the organic and inorganic matters excreted, it was mainly in the amount of inorganic constituents, and was too inconsiderable to be considered an effect of the medicine, while the amount of urea was unchanged, and that of uric acid even diminished.

I had intended to have closed these investigations with comparative experiments on several of the other salts of the alkalies, but they have already occupied so much more time than I had anticipated, that I am reluctantly forced to defer their completion until some future period.

[1] Garrod on Gout and Rheumatic Gout.

[2] I have since taken thirty grains of the carbonate of lithia at one dose, with the effect of rendering the urine passed three hours afterwards strongly alkaline.

ART. VI. — *New Method of treating long standing Dislocations of the Scapulo-clavicular Articulation.* By E. S. COOPER, A. M., M. D., Prof. of Anatomy and Surgery in the Medical Department of the University of the Pacific.

THE difficulty of effecting a cure even in recent cases of dislocations of the scapulo-clavicular articulation without deformity is well known, and in those of long standing treatment has heretofore seldom, if ever, been attended with complete success.

Prof. S. D. Gross says: "Dislocations of the scapular extremity of the clavicle are always attended with difficulty, and it very rarely happens that they get well without some degree of distortion. Prof. Boyer, one of the most able and copious writers upon surgery of the present day, relates a case in which the patient, after five months' regular treatment, could not move his arm without causing a relapse of the complaint."—*Anat. Physiol. and Diseases of the Bones and Joints*, p. 255.

This joint admits of the smallest possible degree of motion appertaining to any joint in the natural condition, and the plan of treatment now to be explained was based upon the supposition that a bony union between the end of the clavicle and acromion process of the scapula could confer little or no inconvenience, but would at the same time remove the deformity; and having long since discarded the favourite doctrine that air admitted to bones produces irritation and a tendency to necrosis, I decided upon the practice shown in the following cases.

CASE 1. Mrs. M. A., a native of Wales, æt. 36, mother of several children, consulted me in 1856 for a dislocation of the acromial extremity of the left clavicle of several years' standing, which had impaired the usefulness of the arm of that side to a very great extent. She had consulted several surgeons in Wales, one of whom used compression upon the elevated extremity of the clavicle for several months without avail. Two years afterwards she consulted some of the most eminent surgeons of London, but without receiving any permanent benefit, though she wore an apparatus recommended by one of them for several months. Subsequently coming to the United States, she consulted different surgeons, none of whom, however, advised any effort to be made to remove the deformity.

She had a nervous desire to get rid of the deformity amounting almost to insanity, and entreated me to do something for her. I proposed cutting down upon the bones, drilling them, and applying ligatures of metal to hold them together. To my surprise she accepted the proposition, not only with promptitude but apparent enthusiasm. All the *supposed* danger of admitting air to bones was fully portrayed to her, as founded upon the authority of the greatest practitioners for hundreds of years, but all had no effect. Her husband and herself would take all responsibility, she said, bearing in mind an operation which had been performed upon her nephew, where metallic ligatures were applied to the tibia, and to which the husband consented.

Although from my experience with ligatures applied to bones I confidently calculated upon success in the case, it was desirable to have the parties fully informed as to what others would say should any untoward result follow, against which we cannot safely guarantee in any operation, however simple, and which is liable to fall particularly heavy upon the surgeon who performs an experimental operation upon principles opposed to the opinions of the profession.

Operation.—The operation was commenced by making an incision three inches long, commencing a little external to the border of the acromion process of the scapula, and following the direction of the clavicle. A transverse incision of two inches was next made, passing over the elevated extremity of the clavicle. The flaps were then dissected away, the bones exposed, their articulating faces trimmed with the bone scissors, and then bored with a drill one line and a half in diameter, about half an inch from the articulating surfaces. The drill was passed through obliquely from above in either bone, the end of the drill being directed towards the opposite one. A wire one line in thickness, made of virgin silver, was then introduced, and a firm knot formed by twisting the ends together immediately over the articulation. Two healthy bony surfaces being brought together and held firmly there, I calculated most confidently upon a speedy osseous union, which turned out to be the case.

Considerable difficulty was encountered in passing the wire from one bone to the other. By having the end well sharpened, however, and bent, after several trials it was passed through the holes in the bones.

After-treatment.—The after-treatment consisted of applying a piece of lint wet with an evaporating lotion (composed of one part of alcohol and ten of water) in the wound around the ends of the wire, which were then bent down over the margin of the wound, over which was placed a piece of wet lint. A roller was then applied over the arm and shoulder of that side as tightly as the patient could conveniently bear. The dressings about the wound were kept constantly wet with the evaporating lotion for four days, when they were removed and a poultice applied, after which the wound granulated kindly, and without an untoward symptom the patient recovered perfectly, without deformity.

At the end of six weeks the wires were untwisted, one end cut off by passing the bone scissors down to the side of it nearly by the bone, when the other end was readily withdrawn.

As soon as the wound began to suppurate freely a movement of the wire was commenced by carrying the ends back and forth. This was done for the double purpose of keeping the wound slightly open for the discharge of any spicule of bone which might be thrown off, and which not unfrequently occurs in all operations upon the bones and for facilitating the final removal of the wires.

The motion of the arm is as good as ever, and at the end of nearly three years there is no indication of a return of the deformity.

Remarks.—There were no sutures used to bring the soft parts together in this case, which is in accordance with my common practice in all operations upon the bones unless the wound be necessarily over four inches long, and in all cases I fill some part of it with lint, so that a communication may be kept up between the surface and the bone operated upon, until it is apparent that there is no longer danger of any portions of bone exfoliating,

which might otherwise be confined deep under the surface and cause great irritation.

The application of lint in the wound, when pressed tightly against its surfaces by means of a roller, is used for these purposes, viz: 1st, to prevent secondary hemorrhage; and 2d, to condense the tissues and produce firm adhesions between all the parts constituting the surface of the wound, so that purulent matter cannot escape into the adjacent tissues; and to accomplish this object a roller applied as tightly as the patient can conveniently bear, is, I think, absolutely indispensable. The lint is permitted to remain in the wound from five days to three weeks; it is generally, however, removed at the end of the first week.

What are the sources of danger and death in all the various operations upon the bones? A few die of a shock to the nervous system, or of immediate supervening local inflammation; but these cases are very rare. Nearly all the suffering and fatality occurring after operations upon the bones arises from the burrowing of purulent matter, either with or without secondary disease of bone. The lingering but severe prostration and fatal constitutional irritation so frequently attending, is nearly always dependent upon this cause, which will in most cases be removed by the plan above recommended.

I do most earnestly commend this matter to the serious consideration of the medical world, because in this city, where such an unusual number of cases of diseases of bone requiring operations are constantly occurring (owing to the habits and avocations of our people), death scarcely ever takes place even in operations upon the femur; while in some parts of Europe one case in five or seven terminates fatally of all the different operations upon the bones of the upper and lower extremities where any considerable amount of bone is removed.

My plan in all these operations is to keep the ends of the bones operated upon in sight until they are covered with healthy granulations, indicating that exfoliation or further disease of bone is no longer to be feared. The confinement of a small piece of dead bone in the centre of a limb would be amply sufficient to cause a very high grade of constitutional and local irritation, and not unfrequently hectic fever and death. Many limbs, and lives too, have been sacrificed from this cause, which would generally be obviated by the plan recommended.

CASES 2 and 3. The first of these was operated upon one year after the one just detailed. The deformity was one of three years' standing. Treatment and results the same as before.

CASE 3 was operated upon in January of the present year (1860). The disarticulation was of six months' standing. Operation, treatment, and result the same as in the first two cases. I am well aware of the great opposition to the above practice which is likely to be raised by members of the profession, in which it is universally admitted that atmosphere admitted

to bones is apt to be a source of great injury to a patient; but I recommend it with confidence, and am satisfied to rest the justification of its claims to value upon the statistic of results in future operations. But the after-treatment must be the same as practised in these cases.

ART. VII.—*Some Account of Diphtheria as it occurred at Oakland College (Miss.), and Vicinity.* By R. H. GOLDSMITH, M. D.

THE latitude of this locality is about 32° N. The country is hilly, and its altitude from fifty to seventy-five feet above the Mississippi River. The soil is clayey, and except in the bottoms, poor. The woods are gradually disappearing, but in the bottoms the undergrowth is rank and luxuriant, with many willows, dogwood, cane thickets, &c. The soil being clayey, retains much moisture: and, although not marshy, is constantly in a saturated condition. In summer, when the thermometer attains an average range of 90°, the most noisome malaria is generated in these bottoms, and is carried over the hills by every wind. This heat continues for three full months, with a fall of the thermometer of 10° to 15° every night, during August and September. As a consequence, during these months the dews are heavy, and the putrefaction of decaying vegetable matter rapid and abundant.

The summer of 1859 was a season of malarial fevers, severe in form, with a typhoid tendency, and extremely difficult to treat. We had a singularly dry summer, although the river had overflowed the flat lands along its whole extent. The winter following presented a series of atmospheric changes, such as the South has never before known; thermometrical records exhibited the highest and lowest temperatures ever recorded in the State. An abundant snow, with the thermometer below zero, were the precursors of the most virulent and malignant diphtherial epidemic ever seen in *any locality*.

Diphtheria has been endemic in this vicinity for three years, and epidemic since January, 1860. It has varied in severity, from the slightest deposition of exudatory membrane, to the complete plugging of the larynx and trachea; yea, I may add, with the deposition of diphtherial membrane from *fauces to anus*. I propose to give the history of a few cases, from a record of three hundred which have occurred in my practice alone. On a single plantation, in January, I count one hundred and twenty cases, and, from this place, I select, at random, cases demonstrative of different shades of the disease.

CASE I. Called on Jan. 2, 1860, to visit Lizzie, a black woman, aged 25, in the following condition: Voice thick and husky, complains of inability

to swallow; eyes red, and capillaries filled almost to bursting; pulse 120; skin dry and harsh, with a most pungent heat. High fever; bowels constipated. On examining throat, I found it covered with an ash-coloured, gangrenous-looking deposit, so thick as to impede respiration, the air passing through, causing a sound like that of a child's whistle. This, I take it, is the ordinary form of diphtheria, allowed to run on to this stage without treatment.

Treatment.—I immediately swabbed her throat with a solution of nitrate of silver (gr. xx to the ounce of water). For this purpose, I use a large and also a very small size probang. The large probang has a sponge, ball-shaped, one inch in diameter, and will take up a tablespoonful of the solution. The small probang I use for washing the œsophagus and larynx, when the deposit reaches these localities. Here, let me remark that, in all my cases, I have never had occasion to regret immediate application of the caustic; on the contrary, the only case that I lost on this place, died because I had no proper instrument with which to make a thorough application of the caustic. My plan has been (and experience confirms my method), to apply the caustic solution as soon as possible, regardless of the amount of deposit. The first application enables the patient to breathe by removing the deposition, and the application is to be reapplied, according to circumstances, the object being to prevent the re-forming of the false membrane. To resume, I also gave the following solution: R.—Saturated solution of chlorate of potash ℥xij; Tinct. sesquichloride of iron ℥ss; Syrup ℥ij. Mix. Take a tablespoonful every two or three hours. She is also to take a teaspoonful of brandy every hour, mixed with a little sweetened water. I have never yet seen a case in which the prompt administration of stimulants was not only beneficial, but *absolutely abortive* in their action on the disease. The fever and complications are of a typhoid character—the poison like that of typhus—and, consequently, the treatment should be tonic and stimulant. This woman was also allowed a liberal quantity of beef-tea during the day, with an opiate at night. This constituted the whole treatment, and convalescence was prompt.

CASE II. A boy named Branch, aged about 16, a stout, healthy negro. He presented that most alarming symptom, a picking at the bedclothes, constant desire to throw off bedclothes; a low, muttering delirium; he passes his feces involuntarily. Pulse 150, compressible, gaseous. Heart labouring violently to propel its blood. He has had ten or twelve dejections during the last twelve hours. They resemble the deposit on the throat, with the addition of blood. Tongue of a fiery red, with a black, dry crust in centre. Respiration reduced to a whistle, varying from 20 to 30 in the minute. Sordes about teeth; fauces covered with the diphtheritic deposit, as far down as I can see. I had no hope of this patient, but I went briskly to work. Washed out the throat *well* with small probang, touched the *rima glottidis*, and far down the œsophagus, and brought up a lump of the deposit as large as a hickory nut, which resembled cotton, painted gray. The boy breathed freer immediately, and, with some difficulty, swallowed. I gave the following: R.—Carb. ammonia ℥j; Æther sulphur. ℥iv; Tinct. zingib. ℥ij; Spts. vin. gall. ℥iv. Mix. Dose, a dessertspoonful in a little water, every half hour.

To check diarrhœa: R.—Plumb. acet. gr. x; Tinct. opii gtt. xx; Starch water ℥ij. Use at once, and repeat every two or three hours, *pro re nâta*.

At 5 o'clock of this day (Jan. 6), the boy is more quiet; operations not so frequent. I reapplied solution of caustic, and gave the chlorate of potash mixture, as in Case I., and the following injection *per anum:* ℞.—Nitrate of silver gr. j; Laudanum gtt. xx; Starch water ℥ij. Mix. Use at once, and repeat in three hours.

I continued a similar course for seven days, adding various tonics as the indications demanded, and giving brandy as freely as the boy could take it. He made a perfect recovery.

CASE III. A little negro boy aged 7 years. I found him with eyes red as fire, spitting mouthfuls of blood; prostrate. I washed his mouth with water, to examine throat, &c., and, to my astonishment, beheld blood oozing from gums, tongue, and whole interior of throat. During his attack he lost, at least, a pint. He had profuse diarrhœa. I may here remark that the worst cases are those accompanied with diarrhœa, as it soon exhausts the patient, and is difficult to check. I pursued my usual plan, using nitrate of silver, brandy, beef-tea, &c.; subcarb. bismuth and morphia to check bowels. This child recovered.

CASE IV. I mention because of a most untoward complication, namely, paralysis of muscles of neck, &c. All symptoms of the disease were checked, deposit had disappeared, and I regarded the little patient (a child aged 8 months), as convalescent. Suddenly the paralysis occurred, and all known remedies were exhausted, without avail. It died.

CASE V. The last case I will mention is that of a mulatto boy, aged about 14. He had all the worst symptoms from the time he was taken sick; was delirious; the secretions from throat at each expiration, were forced through the nose. I had no probang with me, but made a rude one, and applied caustic solution as thoroughly as the instrument permitted. In about eight hours used the proper instrument, with much relief to patient, but he was suffocated by covering his head with the bedclothes, a practice which our negroes have in sickness and health. I sustained him for days before he died, with stimulants by mouth and bowels, and think he too might have been saved, but for the accident.

Remarks.—I believe that this disease is produced by some specific poison, being similar to a disease in cattle, the black tongue. This cattle disease—black tongue—prevailed among the stock of our neighbourhood, whilst diphtheria was raging, black tongue preceding diphtheria. It is certainly a remarkable coincidence that the country adjacent to the plantation on which the black tongue prevailed, was most affected with diphtheria.

On the same plantation, every possible shade of the disease occurred, together with diseases which I regard as cousin-germain, namely, typhoid pneumonia, typhoid fever, and two cases of pure typhus. This plantation is well managed, the houses new, the negroes well clothed, well fed, and yet the whole number of servants—one hundred and fifty—were attacked. They were taken sick just after the Christmas holidays, and after the unusual luxuries consequent on that festival. Among other things, *beef* formed one of the principal staples of their feasts, and this, again, is a striking coincidence, as I took the trouble to ascertain who were the beef eaters, and found they were my first patients.

I regard the disease as *in the highest degree* contagious. Many experiments were made with the view of ascertaining this fact. When an isolated case would occur, I shut the patient in a room with the other sick, and removed all others from the sick quarter. All communication, as far as possible, was prevented between the sick and well, and yet not a single person escaped. Servants from other plantations came to this one, and carried the disease to their homes. I shut them up, in their turn, cut them off from all communication with their fellows, and succeeded in keeping the disease to a single house, when far away from the "Black Tongue" district.

If recognized early, this disease is perfectly amenable to treatment. Since the epidemic, the author has treated two hundred and seventy-five cases, and, of these, but five died, three of which might have been saved if they had been seen early. There are two diagnostic symptoms by which this disease can be surely known, namely, a fiery eye, and a blue, congested appearance of upper lip; the latter symptom is especially apparent among children. A view of throat is, of course, positive.

Treatment must be prompt, immediate, and untiring. Thorough application of a solution of nitrate of silver, of proper strength, at all stages of the disease, and as much brandy as the patient can digest. The chlorate of potash and muriated tincture of iron are also highly useful; the latter, especially, stands high in my estimation, forming as it does almost a specific in an analogous disease, erysipelas. Depressing remedies are worse than useless, nay, accelerate death. Blisters, and all outward applications, are highly injurious; the only one used in this epidemic, a strong solution of iodine, I am not entirely satisfied with, as it sometimes produced frightful ulcerations. If the patient cannot swallow, the following enema may prove beneficial: Port wine ℥j; quinine gr. v to x; beef-tea ℥ij—to be used milk-warm every two or three hours. We are never permitted to despair in this disease; the simple passage of the probang has saved patients who seemed in articulo mortis, and free stimulation perfected the cure. There are many other remedies recommended, and may be tried; the simple course laid down above, has answered for the epidemic seen by the author.

OAKLAND COLLEGE, Miss.

ART. VIII.—*Case of Transverse Fracture of the Patella, successfully treated by means of Malgaigne's Hooks.* By JOHN H. PACKARD, M. D., of Philadelphia.

ON the 28th of November, 1860, I was requested by a friend engaged in an exclusively medical practice to take charge of a lady who had that morning sustained a fracture of the left patella. She had, in going down

stairs with her child, two years old, in her arms, thought herself at the bottom of the flight when she had still a step to descend; she fell, and found that, without to her knowledge the receipt of any blow, the patella had given way. A physician living in the neighbourhood was called in at once, and applied a straight board at the back of the limb, with compresses and a bandage to maintain the fragments in apposition. When her own physician saw her, in the afternoon, the swelling and pain were so great that he directed the application of forty leeches around the joint; the bites bled freely, but there was still great tenderness when I visited the patient with him in the evening.

Upon careful examination, we now made out that the fracture was transverse, and the lower fragment small; it was impossible to define the exact extent of separation of the two portions, but they did not seem to be more than three-quarters of an inch or an inch apart. The compresses were replaced by strips of adhesive plaster applied transversely so as to fix the lower fragment and draw down the upper; a bladder partly filled with pounded ice was laid over the knee, and morphia freely given. This plan of treatment, with very slight modifications, was adhered to for several days.

Mrs. J., the patient, was a very fleshy person, about thirty years of age, and accustomed to somewhat high living. She soon began to feel the effects of pressure upon the bony points as she lay upon her back, and to suffer from constipation. This latter difficulty seemed partly owing to her supine posture. Hence it became very desirable to employ some means by which the coaptation of the fragments should be insured even without absolute quiet.

On the 6th of December, at 11 A. M., eight days after the occurrence of the accident, I inserted the hooks invented by M. Malgaigne for the treatment of these fractures. The inflammation had almost entirely subsided, and I had the full consent of the patient, as well as of my colleague, to the experiment. A good deal of pain seemed to be caused by the application of the instrument, but by the external use of lead-water, and the exhibition of morphia internally, it was assuaged in great measure. The adhesive strips were reapplied, for the sake of safety, and the posterior splint and the bandage were retained. By measurement, the length of the patella was found to be the same as that of the sound bone, the hooks being firmly opposed to one another. At 8 P. M. the patient was free from fever, and there was no evidence of anything unfavourable locally.

On the 11th, five days after the insertion of the hooks, I endeavoured to tighten them by a few turns of the screw; but they were as closely approximated as they well could be. A few days after this, the splint was found to be unnecessary, except as affording a convenient resting-place for the limb; and the bandage was only very loosely applied. On the 21st a trifling degree of soreness and swelling was present, especially about the lower pair of hooks; a rag wet with lead-water was laid over the part, and next day an ointment composed of equal portions of Goulard's cerate and extract of belladonna was applied, after which there was no further trouble.

On the 6th of January, 1861, the thirty-first day from the insertion of the hooks, and the thirty-ninth from the occurrence of the fracture, the instrument was removed. Some swelling of the integuments was apparent, but the bone seemed perfectly solid. The patient was directed to remain in bed, but to use as much motion of the knee-joint as she could, with a view to the restoration of its flexibility.

By the 1st of February Mrs. J. was walking out in the street, having, of course, gained confidence by previous attempts in her room and about her house. She had no sense of insecurity in the limb, and the joint was daily becoming more supple, although, as might have been expected, by slow degrees.

So far as I am aware, the case now detailed is the first one in this country in which trial has been successfully made of M. Malgaigne's plan.[1] The remarks which I have to offer in connection with the foregoing account will have reference, in the first place, to the plan itself; in the second place, to its supposed dangers; and lastly, to its real advantages.

A description of the instrument may not be out of place, since to some of the readers of the *Journal* it may be unfamiliar. The one I used is larger than the one described by Malgaigne in 1847. It is composed of two plates of steel, three-quarters of an inch in width and a quarter of an inch in thickness. Each of these plates is at one extremity recurved downwards into two very sharp hooks an inch and a half long, the points of which are parallel with the plates themselves. The upper plate is two inches and a half in length before the curve of the hooks becomes decided; the lower one is shorter than this by an inch. Projecting up from the lower plate, about three-eighths of an inch from the notch between the hooks, is an upright, which runs in a slit rather more than an inch and a half long in the upper plate; at the extremity of this slit is another upright, fixed in the upper plate, and these two uprights are pierced by a screw which runs parallel to the plates, forcing them, and therefore the points of the hooks, together. The screw is squared at one end, so as to be turned by means of a key.

The lower pair of hooks are to be inserted first, the skin being drawn slightly downwards; they should catch the edge of the tip of the lower fragment, on either side of the ligamentum patellæ. Next the upper plate is slipped on to the lower (which is turned up for the purpose), and its hooks forced in so as to pierce the skin at right angles, and engage the

[1] Dr. Ellis, of Boston, in answer to an inquiry from me in regard to Malgaigne's hooks, says, "They are not liked here, and are never used." Dr. H. D. Noyes, of New York, writes me that in 1854 he saw a case treated with them by Dr. Buck. They produced so much inflammation at the end of two weeks as to require removal. Abscesses subsequently formed around the joint, and the treatment was prolonged to twice the usual period. Bony union took place, but the joint remained quite rigid until the patient was lost sight of. "This case was pretty generally known among New York surgeons, and its results proved condemnatory of the hooks."

points in the upper edge of the upper fragment of the bone. The screw is now passed through the uprights, and turned until the fragments are brought as firmly together as may seem safe. When screwed home, the opposing points are rather less than an inch and a half apart.

All that is necessary to secure the mechanical effect of this instrument is to engage the points properly. The slipping of the upper hooks, spoken of by M. Malgaigne as apt to occur at about the seventeenth to the twenty-second day, did not take place in my own patient, nor do I see how it is possible if the points are rightly applied. One precaution should be taken: to guard the screw by some arrangement of hoops, lest it be subjected to any sudden wrench. Supporting the limb by a posterior splint, although not essential, is, of course, judicious.

In the second place, as to the supposed dangers of this plan. The worst of these would, of course, be that of perforation of the knee-joint by the hooks. But this, as my friend Dr. Brinton and myself have proved by experiment upon the dead subject, is an anatomical impossibility. We not only applied the instrument as for a fracture, but we attempted to drive the points in after the joint had been opened along the lateral edge of the patella. Hence the dread of such an accident in practice may be abandoned.

Another danger that might, *a priori*, be anticipated, is that of ulceration. But my patient had nothing of the kind; there were not three drops of pus discharged from all four of the perforations made in the skin, although around each hook a small circle of induration existed. Nor is there any greater risk of necrosis, since the points do not really enter the bone.

As to the pain caused by such an instrument, it is limited to the few hours succeeding the first application. My patient declared throughout that she felt quite as comfortable as she had when the adhesive strips only were employed. And if necessary, in a nervous or timid subject, anæsthesia might be resorted to when the hooks were put in place.

Lastly, as to the advantages afforded by the plan. It must be obvious that in no other way can we act so directly upon the fragments, or secure them so firmly in apposition; in fact, every other plan is more or less open to the objection that the pressure exerted falls mainly upon the tip of each fragment, so as to tilt the opposed surfaces upwards, and create a V-shaped gap between them. Nor can we by any other means enable the patient to sit up in bed or in an easy-chair, without risk of affecting the closeness of the subsequent union. Moreover, it is easy at any time to test the accuracy of the coaptation, by trying whether the bone and the instrument move as one mass. And finally, we may dispense in a great measure —in fact, almost entirely—with the heating and galling constraint of a firmly-applied bandage, so necessary in most cases of fracture of the patella; or, at least, this may be left off at a much earlier stage of the treatment than when the usual method is employed.

I have dwelt more fully upon the plan proposed by M. Malgaigne, because in the two latest and most authoritative American works bearing upon fractures—the elaborate treatise of Dr. Hamilton, and the *System of Surgery* of Prof. Gross—it is passed over without the slightest mention; and I trust that my success with it may encourage others to careful experiments in its use, so that, if proved worthy upon further trial, it may be added to the therapeutical resources of American surgery.

ART. IX.—*Case of Rupture of the Common Duct of the Liver. Formation of a Cyst containing Bile. Death occurring on the fifty-third day. Autopsy.* By T. M. DRYSDALE, M. D., of Philadelphia. (With two wood-cuts.)

ON the 19th of September, 1859, I was requested by Dr. G. Spackman to visit George Pepper, aged 13 years, who had been injured, several weeks before, by the tongue of a fire-engine striking him in the right hypochondriac region, and forcibly pressing him against a wall, close to which he was standing, and injuring him so that he had to be carried home.

Dr. Spackman saw him about eight hours after the accident. He was then pale, with an anxious expression of countenance, and extremely prostrated. He had vomited a considerable quantity of blood, and was complaining of intense pain in the abdomen, which had seized him immediately after the injury. Dr. S. prescribed morph. sulph. gr. $\frac{1}{4}$, to be repeated every two hours until relief was obtained. After taking three doses of the medicine he slept for several hours. He vomited blood occasionally for three days.

The second day after the accident, his bowels not having been moved, a dose of castor oil was given, which caused an evacuation without any unusual pain, and brought away a large quantity of dark blood. He continued to pass blood with every alvine discharge for several days; and when the feces ceased to be mixed with it, they were nearly white, and ever since have been of a light clay colour.

The first time his bladder was emptied after the injury he passed bloody water, accompanied with a burning pain in the urethra; and for a week following his urine was stained with blood, after which it was very dark, with a green tinge, and extremely muddy.

Dr. S. remarked that the patient was slightly jaundiced on the third day. This increased until he became of a dark yellow colour. He vomited more or less every day, and food produced great agony until it was rejected; it also caused his abdomen to swell very much. He had but little abdominal tenderness, and could bear firm pressure without complaint. Very little medicine was given; in addition to the oleum ricini, already mentioned, he twice took the citrate of magnesia, as his bowels were constipated.

For the jaundice, Dr. S. prescribed the following pills, one to be given every three hours: ℞.—Mass. hydrar. gr. viij; pulv. ipecac, pulv. opii, āā gr. j.—M. et div. in pil. viij. These were followed by a powder composed of hyd. chlor. mit. gr. j; pulv. opii gr. $\frac{1}{2}$; to be repeated every four hours until he had taken twelve.

The abdomen was painted, morning and evening, with the tincture of iodine; but all proved useless; he continued to emaciate rapidly, and the abdomen became greatly distended.

Present Condition.—He is unable to lie down, but sits constantly, propped in a chair. He is extremely restless, and his countenance is expressive of great distress. He cannot stoop without suffering. A deep inspiration occasions intense pain in the right side, below the ribs; "it feels as if something tears." He is very much emaciated, and deeply jaundiced. His skin is cool and dry; pulse 130, feeble and frequent; respiration laborious and hurried. The urine is very dark; it has a green tinge, and deposits a pale yellow sediment. The feces are nearly white.

Examination of the Abdomen.—The distension is very great. The superficial veins are full and prominent. Percussion, above a line running obliquely from the lower border of the eighth rib of the left side to the umbilicus, and curving downward, extending three inches beyond this point, gives a clear tympanitic sound; below this, except along the course of the ascending colon, instead of resonance, there are dulness and fluctuation.

To complete the examination of the abdomen, the catheter was used, and about three ounces of dark urine removed.

Examination of the Chest.—On the left side the lung gives everywhere a loud resonant sound on percussion; the respiration is puerile. The right side of the chest is dull on percussion, anteriorly, as high as the nipple; posteriorly, the dulness extends an inch higher, and over this dull space no respiratory sounds can be detected; above this a feeble vesicular murmur is heard. There is no change in the location of the dulness on altering the position of the patient.

Treatment.—The bowels having remained unmoved for two days, a purgative injection was directed to be given; the application of the tincture of iodine to the abdomen to be continued, and a pill of calomel, half a grain, sulphate of morphia, one-eighth of a grain, to be taken every three hours. For the tympanitic distension ten drops of turpentine to be taken three times a day.

Sept. 20th. The turpentine produced strangury, which was relieved by warm fomentations and opium. There has been no vomiting since yesterday; otherwise the same. As the injection did not operate, we directed it to be repeated; to omit the turpentine and continue the pills.

The urine obtained yesterday was examined, and found to contain bile in abundance. The deposit consisted of the triple phosphate, and of urate of ammonia.

24th. There has been very little change in the symptoms. The vomiting has increased so that his stomach rejects everything but laudanum, of which he is taking teaspoonful doses every three hours. He has vomited some bright blood. The accumulation of fluid in the abdomen is steadily increasing. He suffers intensely.

26th. Condition unaltered. Dr. Washington L. Atlee visited him in consultation this morning, and proposed tapping the abdomen.

4 o'clock P. M. Dr. Atlee performed the operation of paracentesis abdominis. Two gallons of a dark green, viscid fluid, which stained the patient's linen yellow when it fell on it, were removed. As the fluid escaped, the breathing improved; the large veins showing through the skin of the abdomen disappeared; and the tympanitic portion of the enlargement gradually sank, this having evidently been formed by the stomach and the intestines being pushed forward by the fluid behind them. The pulse, which before

the tapping was 130, and scarcely perceptible, had fallen to 108, and become fuller. The patient expressed his satisfaction at the relief afforded.

27*th*. Pulse 100, soft and full; respiration easy. He did not sleep last night; has some tenderness on pressure over the umbilical region; is comparatively comfortable. To have nourishment frequently, and to continue the laudanum. His urine is much lighter in colour.

I examined the fluid removed yesterday by tapping. It was viscid. Its colour in mass was dark green or olive, but when in a thin stratum was yellow. Specific gravity 1018; alkaline reaction. Boiling, and also the addition of nitric acid, produced a coagulum. Sulphuric acid changed its colour to red, followed in a few minutes by green. Nitric acid produced a blue and green colour, which soon changed to gray. Pettenkofer's test produced the characteristic reaction.

Microscopic Examination.—A few shrunken epithelial cells and molecular matter were the only objects seen.

The fluid corresponded in most respects with the description of bile given by Budd, Carpenter, and Simon, but differed from it in containing albumen and being of a less specific gravity. This difference was probably owing to its admixture with the serous effusion from the peritoneum.

28*th*. He is weaker, and continues to emaciate; still vomits his food. His urine is again dark; sleeps very little; complains constantly of pain in his stomach, which even rouses him when asleep; the tenderness of the abdomen continues; no enlargement. To take eight grains of inspissated ox gall three times a day, and to have food in very small quantities, together with injections of beef-tea.

30*th*. The pills have been taken more freely than directed, but the bowels have not been moved. In the evening an injection was given, which brought away ten pills entirely unacted upon. His mind wanders.

Oct. 8*th*. He has continued to sink; the abdomen has been slowly increasing in size. The capillary circulation has been nearly arrested; his chest and arms being quite blue, and in some places having an ecchymosed appearance. He is frightfully emaciated. His stomach rejects the opium; is extremely restless; constant delirium. Directed morphia and beef-essence to be given by injection.

16*th*. Without any change in his symptoms, he continued sinking, and died at 12 M.

Autopsy.—Oct. 17*th*, 4 P. M. The examination was made, twenty-eight hours after death, in the presence of Drs. Atlee, Spackman, Burpee, Fish, Foster, and Dr. Meeteer, of Maryland. The body was extremely emaciated; the skin mottled, and of a purplish colour. Over the chest, abdomen, and various parts of the body, were large ecchymosed patches, which existed during life. The abdomen was enormously distended. On exposing the abdominal cavity, it appeared to be almost entirely occupied by a large cyst, which was in some parts of a greenish colour, in others of a light brown. It was feebly adherent, anteriorly and laterally, to the abdominal walls, as high as an inch above the level of the umbilicus. In handling the cyst, its walls proved to be softened and easily torn, permitting the escape of a large quantity of the same kind of fluid removed by tapping.

On tracing its relations, it was found to dip into the pelvis between the bladder and rectum, being attached to the peritoneal surface of both of these organs by weak adhesions. It filled the abdominal cavity as high as the umbilicus; and on the right side pushed the diaphragm before it, ascended until it reached the nipple, displaced the small intestines, the

cæcum, and the transverse colon, which was folded upon itself, and pressed them all to the left side of the vertebral column. The liver was pushed toward the left side, the right lobe downward, bending it upon itself.

The upward progress of the cyst in the median line was arrested by the root of the mesentery; winding around which, it rose up on the left side, beneath the intestines, as high as the diaphragm, thus forming a tumour of a horseshoe form.

The accompanying drawing represents the parts as they appeared when the abdomen was opened, and will render the above description more easily understood.

Fig. 1.

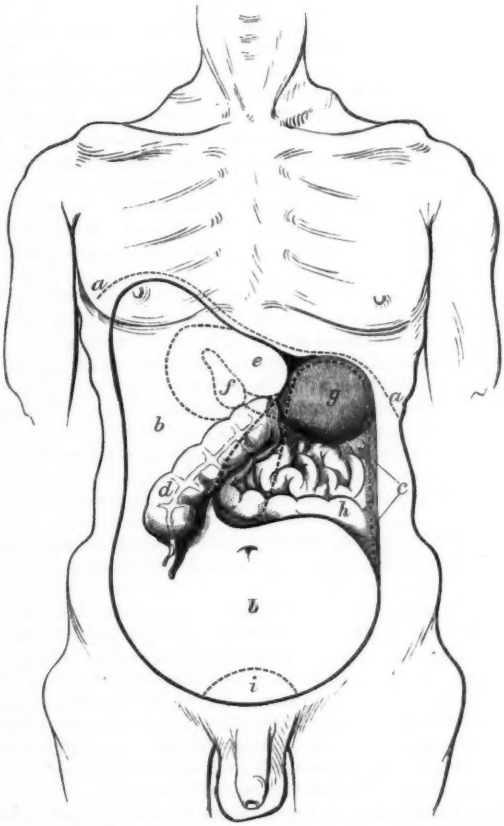


Fig. 1.—*a*. Diaphragm. *b*, *b*. Cyst. *c*. Cyst on the left side lying beneath the intestines and stomach, as indicated by the dotted lines. *d* Cæcum. *e*. The dotted lines mark the location of the liver beneath the cyst, and of the gall-bladder, *f*. *g*. Stomach. *h*. Transverse colon. *i*. Bladder, beneath the cyst.

The transverse mesocolon was spread over the upper part of the cyst, to which it adhered. On the right side, the tumour projected into the epigastric region, anteriorly to the liver, which it concealed, except a small portion of the left lobe, which was firmly adherent by its entire upper surface to the diaphragm. The stomach adhered to the liver, and the pyloric orifice was compressed between this organ and the cyst.

The peritoneum was discoloured wherever it touched the cystic walls, but it presented no signs of inflammatory action, except where the cyst adhered to the surrounding parts. The cyst and liver were so intimately adherent that they were removed together from the body for examination.

The adhesions between the cyst and the liver were broken down until the transverse fissure of the liver was reached, where the cyst became continuous, in part, with the peritoneal surface of this organ. The gall-bladder was found beneath the cyst, filled with bile, and of the normal size.

A careful dissection showed the tumour to be a continuation of the peritoneal covering, both of the liver and of the ruptured ductus communis choledochus; the rupture occurring three-quarters of an inch below the cystic duct. The ragged end of the duct projected a quarter of an inch into the cyst. (Fig. 2, *b*.) The tumour was lined over the greater part of its internal surface with a layer of lymph.

Fig. 2.

Fig. 2 represents a section of the upper part of the cyst on the right side. *a*. Interior of cyst. *b*. Remains of the common duct.

Remarks.—This enormous cyst was probably developed in the following manner: The blow from the tongue of the engine ruptured the ductus communis choledochus, thus permitting the bile to enter the gastro-hepatic omentum, which incloses this duct in its right border, causing inflammation of this portion of the peritoneum and sealing it to the neighbouring parts. The bile, which was subsequently secreted, was poured into the new receptacle, gradually enlarging it until it displaced the viscera and attained the size found after death.

A remarkable feature in this case is the extraordinarily rapid *growth* of a small portion of serous membrane; for we cannot regard it as a mere stretching of this membrane, as the cyst-walls were actually thicker than healthy peritoneum.

The case also demonstrates the power of the muscular coat of the gall-bladder to resist distension, the force of which must have been very great.

In searching authorities for a similar case, I have met with but one having any resemblance to it. This is found in Cooper's *Surgical Dictionary* (Amer. edition, p. 498), and is extracted from the report of Mr. Fryer, of

Stampford, published in the *Med.-Chir. Transactions*, vol. v. p. 330. In the symptoms during life, it so closely resembles the above case that I have transferred it to these pages, both for the sake of comparison and to encourage to persistence in treatment in any like accident, despite the apparently fatal character of the injury.

"A boy, about thirteen years old, received a violent blow from one of the shafts of a cart on the region of the liver. The injury was succeeded by pain, frequent vomiting of bilious matter, great sinking, coldness of the extremities, and a weak, small, fluttering pulse. The belly was fomented, and purging clysters thrown up. On the third day, symptoms of inflammation began, attended with considerable pain about the liver, great tension and soreness of the abdomen, and frequent vomiting. The pulse was quick, small, and weak; the skin hot and dry; the tongue much furred; the urine high coloured; and there was some difficulty of breathing, and great thirst. Eight ounces of blood were taken away, the fomentations continued, and a few grains of calomel were directed to be given every four hours, until the bowels were properly opened. Afterward, the effervescing mixture, with ten drops of laudanum, was exhibited every four hours.

"On the following day, the patient had some motions, and was much better; but, as his sickness continued, he was ordered a grain of opium every four hours. About a week afterward, he complained of a great increase of pain, which was somewhat relieved by a blister. He was now completely jaundiced, and his stools were white, but the tension, pain, and sickness were abated.

"Two days afterward, a fluctuation was perceived in the abdomen, which, in another week, became considerably distended with fluid. The patient now did not complain of much pain, but appeared to be sinking fast. A puncture was made in the swelling, and thirteen pints of what appeared to be pure bile were evacuated. The bowels then soon became regular, and the appetite good. In twelve days, the operation was repeated, and fifteen pints of the same bilious fluid were drawn off. Nine days afterward, another puncture was made, and thirteen pints more let out; and six were discharged in another fortnight. From this period the boy went on well, and perfectly recovered under the use of light tonic medicines."

This case is reported as rupture of the gall-bladder; but, as the patient recovered, the lesion from which he suffered is doubtful, and might, as far as the symptoms indicate, have been one of partial rupture of the common duct.

TRANSACTIONS OF SOCIETIES.

ART. X.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1861. *Jan.* 2. *Large Interstitial Cancerous Tumour of the Impregnated Uterus, rendering Delivery Impossible.*—Dr. WM. V. KEATING read the following account of a very remarkable case of this kind:—

I was requested to visit Mrs. J. in the month of July, 1857. She had been married on the 19th of May previous, and had perceived a slight menstrual discharge on the 22d of the same month, since which time she had no signs of menstruation. Being requested to inform her at what period she might expect her confinement, I calculated that her labour might supervene at any moment after the 24th of February.

Mrs. J. was a native of South America, and when I saw her was in her 30th year. Her complexion was of an olive tint; temperament nervo-bilious. She had previously enjoyed good health, having in fact suffered from none of those ailments incident to her sex at the period of puberty. In the year 1853, without any appreciable cause, in the interval of a menstrual period, she was suddenly seized with uterine hemorrhage, which, though short in duration, was quite active in character. Her attendant, the late Dr. Jos. Nancrede, was called in, and by means of rest and the usual remedies was soon able to arrest it. She never had a return. Mrs. J. subsequently suffered from impaired vision, and had been under Dr. Sichel's care in Paris; and although his directions were strictly carried out, she could seldom indulge in reading, writing, or sewing.

The peculiarity of Mrs. J.'s complexion had forcibly impressed me upon my first visit, and elicited from me many inquiries as to her previous health and the history of her family. I could not but feel some apprehensions in reference to the existence of some cachexia, and my solicitude was only quieted by the assurances of her friends, to whom I communicated my fears, that the peculiar tint resembling cirrhosis, which attracted my attention, was natural to her. Upon questioning her maid, after her death, I discovered that her menstrual discharge had generally been scanty and emitted a very fetid odour, such as she had never noticed before, although in other respects it had the usual characteristics. Mrs. J. enjoyed remarkable health during the period of gestation, and was unusually active, walking often six miles a day, for inasmuch as her weakness of vision prevented her from attending to the usual occupations at home, she was forced to resort to out-door amusement. My sole attendance during her pregnancy was for a violent pain in the anterior portion of the left thigh, an inch above the patella. The pain was very severe, and seemed to cause her great annoyance; it was paroxysmal in its character, and was finally relieved by repeated vesications and endermic applications of the sulphate of morphia.

On Sunday, the 28th of February, at 9 A. M., I was summoned to visit Mrs. J. She had been seized with acute pains in the uterine region, be-

tween 1 and 2 o'clock A. M. About 4 A. M., during one of her pains, she felt something give way, and immediately there followed a gush of a greenish fluid. These pains continued accompanied by a discharge of some bloody mucus until I reached the house, not increasing in intensity or rapidity, but always attended with a dribbling of the greenish-coloured fluid. I immediately took advantage of the access of a pain to make a per vaginam examination. Upon reaching the os uteri, I found it, to my utter astonishment, entirely closed with a cervix uteri as undeveloped as in the sixth month of pregnancy. The os felt hard and not well defined, the cervix almost cartilaginous to the touch, and totally unlike anything which I had ever observed in the organ at this period of pregnancy; repeated and accurate examinations soon revealed the fact to me that the pains had no effect whatever upon the os uteri, whose dilatations had not as yet commenced. I made the usual examination of the straits of the pelvis, and determined that that organ was in every way normal, and hence feared no difficulty in that quarter. My patient was of course in an exceedingly nervous condition, and most anxious to ascertain my diagnosis and prognosis. The pains having had no effect upon the os uteri, and seeming to diminish in intensity and frequency, I deemed it most prudent to state that she was not as yet in labour, but that probably true labour-pains would soon supervene.

The advent of the estimated period of labour, the supervention and continuation of regular uterine contractions with the rupture of the membranes, viewed in connection with the existence of an undeveloped cervix and a perfectly closed os uteri, aroused serious apprehensions in my mind as to the probable existence of some obstruction in the labour or malposition of the fœtus. I requested a careful external examination of the abdomen. I found the abdominal parietes presenting all the tension of the full period of gestation; the uterus had also subsided, and inspection revealed an abdominal tumour of singular shape, the longest diameter of which corresponded to a line drawn from the right hypochondriac region to the anterior-superior spinous process of the left ileum. Careful palpation and even inspection revealed a sulcus or line of separation in the tumour, commencing on the left side about four inches below the last false rib, and extending obliquely down to within an inch of the symphysis pubis, and dividing the uterus into two portions. That portion of the tumour towards the right side was quite prominent; towards the left iliac region a less prominent orbicular mass could be felt, resembling the fœtal head. During the contractions of the fundus and body of the uterus, which I could distinctly feel with my hand laid on the abdomen, the right and more prominent portion of the tumour seemed to undergo forcible contractions, but the orbicular mass to the left seemed in no ways affected, and always maintained its primitive position and condition.

Auscultation at this period revealed very feeble pulsations of the fœtal heart in the hypogastric region, a little to the right side.

I had serious misgivings as to the nature of the case. The above examination convinced me that there existed probably a twin pregnancy with a malpresentation, complicated with premature rupture of the membranes and a rigid os uteri. As the bowels had not been moved for forty-eight hours, I prescribed a dose of oil, and left. Upon my return at 2 P. M. I found that the pains had entirely ceased, but the discharge of liquor amnii still continued whenever she moved. I made another examination, and found everything in the same condition. The os uteri being closed, the pains

having subsided, there was nothing to do but to revive the drooping spirits of my anxious patient, whose extreme confidence in the correctness of the calculation of her period of labour, caused her to suspect that there was some serious cause retarding its progress. The existence of a cervix uteri, the entire closure of the os uteri, the subsidence of all pains, were sufficient considerations to lead me to doubt the accuracy of the computation of her pregnancy; hence I deemed myself justified in asserting that she could not have reached the full period of gestation, and having thus succeeded in calming her fears, I left her.

I visited Mrs. J. on Monday the 1st of March; found her sitting up, and quite refreshed in body and mind, having passed an excellent night, without any return of uterine contractions. The liquor amnii was still oozing from her, and she was annoyed with a constant desire to micturate; she was slightly feverish, but not more so than might be expected in one of such a nervous temperament, annoyed by the sudden subsidence of her pains, and most anxious for labour to supervene. She continued very much in the same condition until the 14th of March. Auscultation, however, never revealed any sound of the fœtal heart after the 28th of February. The dribbling of the liquor amnii continued till the 4th of March; the colour of the fluid then changed to a dark mahogany, with a fetid odour. Occasionally slight paroxysms of fever occurred, but generally as the result of her extreme restlessness and anxiety. During this period there would also occur now and then slight paroxysms of pain, but very feeble and of very short duration, and upon palpation during these pains I could invariably discern that the contractions were confined to the right and more prominent portion of the tumour. Repeated vaginal examinations afforded always the same results, the same conditions of the os and cervix uteri. About the 8th of March the uterine tumour assumed such a position as to cause great inconvenience and at times much suffering. Locomotion was almost impossible, and when the patient was seated the uterine tumour seemed to project over the symphysis pubis, and, as the nurse stated, seemed to rest on the upper portion of her thighs; the legs and feet became quite œdematous. The desire to micturate was incessant, but the urine was scanty, high-coloured, and slightly albuminous. On the 10th of March slight labour-pains occurred, with no effect upon the os or cervix uteri, and after a few hours subsided; the discharge became much more fetid, paroxysms of fever supervened, and so much restlessness and despondency ensued as to force me to resort to anodynes to calm her and to produce sleep. Finding that no change took place, and feeling serious apprehensions in reference to her state of mind, I requested a consultation on the 14th of March. Prof. Meigs visited my patient at 12 o'clock that day, and after a careful examination both internally and externally, fully coincided in my opinion, viz., that the condition of the os and cervix uteri and the absence of all true uterine contractions producing dilatations of the os, with the impossibility to feel any motion of the fœtus, authorized the conclusion that our patient could not have reached the full period of gestation. The extraordinary form of the uterine tumour induced Prof. Meigs also to suspect that there might either be a transverse presentation of the fœtus, uterine pregnancy, a bicorned uterus, or a Ritachristina. As the result of our consultation, we endeavoured to calm the fears of the patient and her friends, and resolved to wait and abide the indications of nature.

Mrs. J. continued in the same condition until the 16th (Wednesday), save a change in the colour and consistence of the discharge, which con-

sisted now of a pink-coloured grumous matter with a peculiarly fetid odour. Upon palpation of the abdomen, I could often discover contractions of the right portion of the abdominal tumour, without, however, any responsive action in the cervix or os, and without occasioning much pain; her state of mind was truly alarming—there were frequent febrile exacerbations, attended with a highly furred tongue, harsh dry skin, and copious discharges of mahogany-coloured urine, which yielded a considerable amount of albumen to the proper tests. She was kept on saline draughts, and under the influence of Dover's powder.

On Wednesday morning I found her suffering from well-defined uterine pains, recurring about every fifteen minutes; upon palpation I discovered that coincident with these pains there were powerful contractions of the right portion of the tumour, but the orbicular mass situated in the left iliac region seemed completely inert and in no way affected by the uterine contractions. These labour-pains had supervened about two o'clock in the morning, and having increased regularly in intensity and frequency ever since, I was led to hope that a per vaginam examination would reveal a decided progress in her labour. At 10 A. M. I made a per vaginam examination. I found the canal very hot and dry, the os uteri seemed distant, and it was difficult to reach it; it was slightly opened, about one-eighth of an inch; cervix uteri still present, but the anterior lip of the os and left portion of the cervix uteri seemed exceedingly indurated, almost cartilaginous. No portion of the fœtus could be felt, and consequently it was absolutely impossible to diagnosticate presentation or position. Bowels being confined, and there being considerable febrile excitement, I prescribed a dose of castor oil and effervescing draught. Returned at 1 P. M.: pains continue increasing in intensity; os uteri opened a little more, and in pushing my index-finger well up into what seemed to be the cervix uteri, I thought I discovered an intestinal-like protrusion of the membranes; repeated examinations confirmed my opinion; I requested Prof. Meigs to visit her with me at 2 P. M. He coincided in my diagnosis, and suspecting the existence of a transverse presentation, recommended me to keep a strict watch so that I might seize the proper moment, rupture the membranes, and attempt version by the head. I remained near the patient all the afternoon, hoping to be able to carry out our design; the pains continued, not very intense, but of a most harassing character; no more effect was produced upon the os uteri or the presenting portion of the child. At 8 P. M. we held another consultation. Ordered anodyne enema and saline draught.

Thursday, 17*th*, 9½ A. M. Met Prof. Meigs. Our patient had a calmer night, and slept about fifteen minutes at a time. The pains continued during the night, but were now much more irregular and less intense; febrile action continues; discharge from vagina diminished; tongue much furred; breath extremely fetid. She was very fretful and irritable; form of tumour remains the same; no change in condition of os uteri; urine still very dark and albuminous. Ordered small doses of hydg. cum creta, to be followed by oil.

I returned at 2 P. M. She had two bilious evacuations and seemed much relieved, and was much more cheerful; skin moist; pains almost disappeared; no change in uterus; slight discharge of a grumous character and very offensive. Relished her dinner, and seemed inclined to sleep. Consultation held again at 6 P. M. She seemed more cheerful; had a refreshing sleep of four hours; pains slight and not frequent; the contractions still continued in right portion of the tumour. Anodyne enema repeated.

Friday 18*th*. Held a consultation at 9½. Passed a restless night; pains returned at regular intervals, and, although not very intense, were of a harassing character, causing her to be fretful and despondent; no change in internal parts; tongue furred; febrile action continues; breath very fetid. Ordered oil. Met again at 1 P. M. Oil operated once; pains of a slow lingering character; patient very despondent and entreats for relief; the presenting point of fœtus has made no advance, and os uteri remains in the same condition. We decided upon giving 10 grs. of ergot every hour, and I remained with the patient to watch the effect. After the fourth dose the pains increased in frequency and intensity, and were of a most distressing character; the right portion of the uterine tumour contracted so actively that I at one time entertained serious fears that rupture of the uterus might occur, but the left orbicular mass still seemed perfectly inert, maintaining all its former characters with the sulcus remaining perfectly defined. We met again at 6 P. M. Pains still continued very violent. After a long and careful examination, Prof. Meigs thought that he felt a small portion of the fœtal head compressed within the cervix uteri, and concluded that what we had repeatedly considered an intestinal-like protrusion of the bag of waters was really a portion of the softened putrescent scalp. In an hour after this view was confirmed by the possibility of detaching portions of the scalp with the finger, we decided to allow the pains to push the presenting portion further down, and appointed to meet at 10 P. M.; but finding, at 9, that her pains were almost insupportable, and her entreaties for relief so urgent, I went to Prof. Meigs and requested him to bring his craniotomy forceps and blunt hook. We decided that although the os uteri was not dilated more than an inch and the cervix undeveloped, still, on account of the agony which she was enduring, we would be justisfied in making careful and gentle traction upon the presenting portion of the head, and thus force it to engage within the circle of the os. This plan of action was based upon the fact that her violent pains seemed to produce no effect, and believing that we had either a monster with two heads or a transverse presentation which nature could not disengage, we deemed it prudent and humane to assist by gentle traction efforts in forcing the presenting portion to engage. The difficulties we had now to encounter beggar all description. The extreme distance of the presenting surface, the exquisite sensitiveness of the canal, the tenesmic resistance of the os uteri, the difficulty in seizing and keeping hold of the small portion of bone which could be reached, seemingly imbedded in the tissues of the cervix uteri, presented such insurmountable obstacles that, after repeated efforts, attended solely with the crumbling and detachment of minute portions of bone, we were obliged to desist about 11½ P. M., especially as the patient had been for so long a period of time under anæsthesia. The condition of the os uteri under this operation was one which caused us the greatest annoyance and apprehension. It was utterly impossible to pass the finger around the anterior and left portion of the os, the presenting bones of the fœtal head seemed imbedded in the substance of the uterus itself, and every traction effort was attended with the sinking of a rough saw-like edge of bone deeper and deeper into the tissues of the cervix and lower part of the body of the uterus; posteriorly and to the right the finger could be introduced as far as its length would allow, but to the left the cartilaginous condition of the os and cervix uteri and the steady resistance of the orbicular mass seemed to defy all efforts at delivery. The patient was exhausted, the vagina had become intensely swollen; we were, therefore, forced to desist. An opiate

draught was administered, and Prof. Hodge sent for. We all three met at midnight. Prof. Hodge examined and coincided with us as to the condition of things. We determined to allow the patient a certain period of repose, and then, under the influence of anæsthesia, to make further attempts at delivery. A powerful opiate was then administered, but the constant uterine contractions, superadded to her extreme anxiety, antagonized its effect. Nourishment and stimulus were administered, and at 2 A. M. Prof. Hodge attempted to extract the presenting portion of the fœtus; but after the most powerful efforts, finding that no progress was made, we deemed it more prudent to keep the patient under opiates, and to soothe the vagina and os uteri with demulcent and antiseptic injections.

Adjourned our consultation to Saturday, 19th, at 9 A. M.

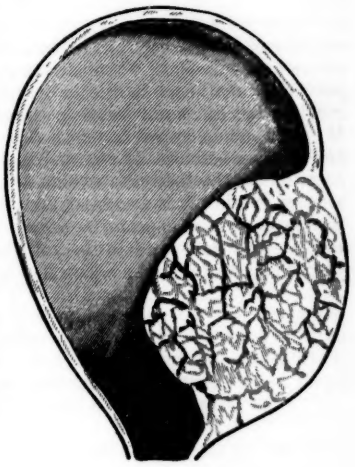
Mrs. J. did not sleep; uterine pains incessant; tongue very much furred; pulse 140; abdomen tympanitic; urine scanty and albuminous, and has to be drawn off by catheter; vagina in a high state of inflammation, and the discharges so fetid as to affect every one in the room. Prof. Hodge suggested that we should attempt dilating the os uteri by the application of Kiwisch's uterine douche; this was tried eight hours without any success, the pains became much more violent under its employment. Colpeurysis was also resorted to. Our patient was now kept under the effects of powerful opiates and a supporting diet. Her condition grew worse and worse every hour, and her agony was such as to fill with anguish all those who surrounded her. New instruments were devised, constant attempts made to effect delivery, but all that science could suggest and art devise seemed totally inefficient.

On Saturday, at 2 P. M., Dr. La Roche was added to the consultation, and the subject of the Cæsarean section was proposed, considered maturely, and rejected on the following grounds. The greatest authorities who have written upon the Cæsarean operation have considered it to be necessary: 1st. In the case of extra-uterine gestation. 2dly. Where the fœtus has escaped entirely into the cavity of the abdomen on account of rupture of the uterus. 3dly. In the case of hysterocele or hernia of the uterus. 4thly. Where tumours, bridles, cicatrices, adhesions, or other affections of the soft parts included in the pelvis obstruct this bony canal, the neck of the womb, or vagina. Lastly. Where the pelvis is in itself so far defective that there no longer exists between its dimensions and those of the head of the fœtus the relation necessary to delivery. After the most careful examination and consultation, not being able to discover the existence in the present case of any of the foregoing conditions, we deemed that the ethics and the acknowledged rules of practice forbade us attempting the operation. Moreover, it was unanimously agreed that, under Mrs. J.'s present condition of intense constitutional excitement, the operation would be necessarily fatal. We were, therefore, constrained to adopt an expectant treatment and to alleviate her pangs as much as possible, hoping from hour to hour that nature might effect some change in her behalf. Finally symptoms of pyæmic intoxication set in and soon banished from us all hope of saving our patient. The uterine pains continued incessantly up to the period of her death, which occurred on Friday the 25th, at 2 P. M. To narrate her symptoms, to describe her agony, or to express the feelings of those whose sad duty called them to her bedside without the slightest power on their part to relieve or deliver her, would be recalling scenes and pangs which, we trust, our professional career will never cause us again to witness and endure. Suffice it to say that our poor patient died in the violent throes of labour, having

suffered uterine pains for ten days and nights with scarce an intermission save the natural intervals between the pains. Those among us who are familiar with the characters of uterine contractions can well imagine the horrors of these ten days of agony to this poor lady, and can well appreciate the gloom and despair of her medical attendants in beholding them.

Post-mortem examination was held nineteen hours after death. Performed by Dr. Ellerslie Wallace, in presence of Drs. Meigs, Hodge, La Roche, Sr., La Roche, Jr., and Keating. Upon uncovering the abdomen the sulcus in the external portion of the abdominal parietes still quite apparent. On removing parietes of abdomen the uterus presented the sulcus, but higher up than it was externally. There were no signs of parietal or intestinal peritonitis. Uterus 10½ inches long and lying obliquely; above the sulcus measures 7½ inches; at the sulcus 7 inches; two inches below sulcus uterus measured 7 inches. The fœtus had never entered the superior strait, and was in a complete state of decomposition. On removing the uterus it was found that the os uteri was almost cartilaginous in consistency and barely large enough to admit the introduction of two fingers. Upper portions of vagina and lower half of cervix uteri in a state of sphacelus. The tissue of the uterus near the cervix was extremely congested. On opening the uterus the lower globe of uterus was found to be an immense interstitial tumour. (See fig. from a drawing by Professor Meigs.) Transverse diameter of the tumour included the whole os uteri; antero-posterior diameter of tumour 5½ inches; vertical diameter 6½ inches. General aspect of tumour ovoidal. Placenta attached to the posterior aspect of the fundus uteri. General surface of the uterus in a complete state of putrescence.

Fig. 1.



Microscopical examination of the tumour by Dr. Da Costa.—I annex the results as embodied in a note to me: "The specimen you sent me for examination was peculiar in several respects, and especially from the fact that the morbid material did not exist by itself, but had become intimately blended with the proper structure of the uterus. Under the microscope I found infiltrated between the smooth muscular fibres of the womb, cells with distinct nuclei, some small, some large, and obscured by granules, but entirely resembling those of cancerous masses. In addition, there were some oil drops and a large number of granules. The muscular fibres were here and there altered in appearance, infiltrated with granules, yet on the whole I was struck with the fact that, whilst so much diseased action had been going on around them, so many of them, by far the largest majority, had remained perfectly unaltered in shape, size, and contents. I have no doubt that the tumour is cancerous; softening had commenced."

The sad result of this case, and the light shed upon it by the post-mortem examination, will undoubtedly suggest to many the propriety, under such circumstances, of resorting to the Cæsarean operation. In answer I would merely recall to your memory the anecdote of Columbus and the egg.

It has undoubtedly created some astonishment in the minds of many here present that this unfortunate lady should have been allowed to die undelivered, especially as the statistics in reference to the Cæsarean section had in latter days been so favourable. It would be an injustice to myself and respected colleagues did I not recall the fact that with the data before us it was absolutely impossible to anticipate or diagnosticate the nature of the obstruction which we had to overcome. I believe that it must be conceded that the most skilful and experienced acumen in diagnosis might be expected to fail on occasions like the present. To justify a resort to the Cæsarean operation there must be present some of the conditions which I have enumerated in another portion of this paper, failing to discover them, and being convinced that the nature of the obstruction was such as to induce us to hope that it might yield from hour to hour, in the doubt and obscurity in which we were groping, we felt bound to act with that conservatism which eschews all hap-hazard treatment, having neither reason nor experience for its basis. "Melius anceps remedium quam nullum" may be a justifiable apothegm in the judgment of the charlatan, the ignorant, and reckless, but can never prove a safe guide for the conscientious physician. When it became apparent that a delivery, *per vias naturales*, was impossible, the case had already assumed such an aspect from pyæmic intoxication as to render the Cæsarean section not only useless, but, under such circumstances, cruel. It is a sad satisfaction to us, in reflecting over the results of the post-mortem examination, to find that even in this unprecedented case a strict adherence to the rules and ethics of our art has proved the justice and wisdom upon which they are founded.

Art. XI.—*Summary of the Proceedings of the Pathological Society of Philadelphia.*

1860. *Nov.* 14. *Completely Adherent Pericardium.*—Dr. Charles C. Lee presented this specimen, which was taken from the body of a patient who died in the Blockley Hospital.

The subject, a coloured girl, æt. 20, had been for two years an inmate of the hospital, suffering from hypertrophy and aortic valvular disease, with occasional attacks of pericarditis, but her general health was sufficiently good to enable her to go about the house. One evening she was suddenly seized with dyspnœa, and violent pain and palpitation in the cardiac region, in which condition she expired in a few moments, before any physician could reach her.

The autopsy was made twelve hours after death, by Dr. Weidman, who informs me that when he opened the thorax nothing was visible but an immensely enlarged heart, which appeared to have gradually compressed and displaced the lungs. The heart was more than double its usual size; and so completely obliterated was the pericardial sac, that at no point could the membrane be separated from the cardiac walls. There was marked insufficiency and thickening of the aortic valves, but no atheromatous or other deposits; the mitral valve, with its fleshy columns, was also much thickened. All the cavities of the heart were greatly dilated, and its walls hypertrophied. The lungs were smaller than usual, rather congested, and felt almost solid to the touch. The other viscera were healthy.

Dr. Packard referred to a case of a similar kind, reported by him to the Society about three years ago. (See Proceedings, vol. i. p. 52.) In that case, as well as in the specimen now presented by Dr. Lee, there had been insufficiency of the auriculo-ventricular valves, and adhesion of portions of them to the ventricular walls. It would be interesting to ascertain in what proportion of cases of adherent pericardium this fastening down of one or more flaps of the auriculo-ventricular valves is present.

Internal Hemorrhage in a Newly-born Infant.—Dr. Keller exhibited the respiratory organs, and said: The mother of the subject from whom these specimens were taken was a strong, masculine woman, æt. 38 years, who, after eight years of married life, was for the first time taken in labour on Friday evening, the 26th of October last. Her pains continued without remission until 4 P. M. of the following Sunday, when she seemed so much exhausted that I deemed it advisable to deliver her with the forceps. I was obliged to detach the placenta, a portion of which was slightly adherent, and this operation, though performed with great care, was followed by considerable hemorrhage, which was with difficulty arrested. The child showed but faint signs of life; and was only resuscitated after strenuous exertions on the part of the attendants, when it threw up a quantity of greenish mucus mixed with blood. For a short time after this its respiration continued regular, but soon became laboured. All the other functions were normally performed; but on the following morning I noticed that the abdominal veins were enlarged, and the abdomen itself much distended with flatus. The child died forty hours after its birth.

Autopsy, twenty-four hours after death.—The mucous membrane of the trachea was found deeply tinged, and covered with thin liquid blood. The lungs floated in water, but were highly congested, especially the left at its upper lobe. The heart contained a small quantity of fluid blood. The stomach contained a quantity of dark coagulated blood, mixed with mucus; the intestines and other viscera were healthy.

Cerebral Hemorrhage.—Dr. ROBERT P. HARRIS reported the following case of apoplexy, occurring in an anæmic patient: On Tuesday morning, October 20, 1860, I was hastily summoned to see Mrs. S., aged 34; the mother of four children ranging from five years and a half to six months old. When I reached her bedside she informed me that she had been suffering with a severe pain in the right side of her head since the previous Friday, accompanied by noises in the ears and an occasional numbness in the right arm and leg; that she had taken purgative medicine, under a belief that her symptoms proceeded from indigestion, but without relief; and that just before I was sent for (at 9 A. M.) she had fainted and fallen upon the floor in a state of unconsciousness, soon after coming up-stairs to her chamber, but had in a little while recovered. I found her very pale and weak, with a slow, regular, but feeble pulse, and suffering from a violent pain in the right side of the head. Her consciousness was perfect, eyes natural in appearance, and muscular movements unaffected. Her chief complaint was of headache and extreme debility. Previous to the attack in question she had suffered from weakness for a long time, consequent upon too frequent childbearing and the necessity of constant nursing, together with attacks of epistaxis, to which she had been subject from early childhood, preceded in latter years by hyperæmia capitis. In less than six years she had given birth to four living children, one at a time, and had twice miscarried. Previous and subsequent to her last accouchement she suffered very much with a catarrhal affection, and had frequent attacks of faintness and supra-orbital neuralgia. She was in person spare, of medium height, had a long neck and narrow head, and, when in moderate health, had a bright red blush upon the upper part of her cheeks. She was subject to frequent and obstinate coughs, and her appearance might have led one to fear tuberculosis.

The last time I saw her, previous to this attack, was in the last week in September; and upon that occasion I warned her of the danger to her health to be expected from continuing to nurse her infant, and ordered her to wean it. From that time up to the attack of headache on Friday she had been apparently improving in health, and a cough which she had had for several months had left her. She had not, however, derived all the benefit that she might have had from weaning her infant, for, although it was hand-fed during the day, she allowed it to nurse at night. The day before that upon which she fainted (Monday) she had a severe pain in her back, seated in the lumbar region, and discharged what, judging from description and other signs to be hereafter referred to, must have been a blighted ovum of the first month. Its exit was unaccompanied by any hemorrhage; and her mother, who informed me of the circumstance, did not know whether the body, which she described as oval and of a fleshy appearance, came from the vagina or rectum.

About 10 o'clock, soon after I had left the patient's house, she was seized with convulsions, and the messenger failing to find me, Dr. Hoyt was called in, from whom I have received the following statement of her symptoms

during his visit. She had several spasmodic attacks, lasting some minutes each time, the motion being confined to the right arm and leg. The movement of the arm was not jerking, as we generally see it in the convulsions of children, but was evidently, to a certain extent, voluntary, as the hand was applied to the right side of the head, over the seat of pain, and passed backward and forward over the side of the head, face and neck. The conjunctivæ were slightly injected, and the pupil of the right eye was largely dilated, that of the left being much less affected. After the cessation of spasmodic movements, and during the interval between the attacks, the difference between the pupils was much less marked; both, however, being still slightly dilated. The absence of motion on the left side of the body during the spasms was evidently not due to paralysis; as voluntary movements of the left arm were made during the intervals. The muscles of the eyes and face were unaffected, as well as also those of the neck. During the spasms the patient breathed heavily, with strong stertorous respiration; but in the intervals there was only a very slight snoring sound produced, and she appeared to be in a profound sleep.

At 1½ o'clock P. M. I again saw the patient. She was then apparently asleep; pulse 60, regular and feeble; respiration slightly snoring; right pupil a little more dilated than the left, but the enlargement of neither was much marked. When roused, and asked if she felt pain in her head, she assented with a nod, and was soon afterwards seized with vomiting, the contents of her stomach being ejected with considerable force; after which she relapsed into unconsciousness, and remained in this state until about 4 P. M., when she was seized with a spasmodic contraction of the arms and legs, and in a few minutes expired, just seven hours after the fainting fits took place.

Autopsy, made twenty hours after death.—Body well formed, but quite spare; surface remarkably pale; rigor mortis well pronounced; extremities contracted in a peculiar manner. The forearms were pronated, and slightly flexed on the arms, the hands flexed upon the forearms, the fingers curved and adducted, and the thumbs flexed and turned into the palms, as we see them in cases of laryngismus stridulus. The feet were drawn inwards by a contraction of the tibialis anticus muscles, so that the soles were presented towards each other.

Upon removing the skull-cap, the surface of the left hemisphere of the cerebrum was seen to be intensely congested, particularly at a point immediately above the fissure of Sylvius. The dura mater being opened, a little serum escaped from the left hemisphere. A horizontal section made above the corpus callosum revealed the presence of a large, irregularly-shaped cavity in the centre of the right hemisphere of the cerebrum, filled with a soft black clot of blood. This cavity, which had no communication either with the fissures of the cerebrum or the ventricles, had a capacity of about three fluidounces. The cerebral substance around the clot was very much softened, and easily broke down under the handle of a scalpel; its surface was studded with numerous points of blood. Upon dissecting up the ramifications of the cavity, it was found to have been formed from below upwards and backwards, commencing just above the fissure of Sylvius, in the middle lobe of the cerebrum, and extending up into the centre of the hemisphere, forming at the upper part of the cavity a rounded excavation, which we first cut into in making the section referred to. Upon tracing out the bloody track at the bottom of the cavity, the effused blood was found to have come from a rupture of the middle cerebral artery.

The left hemisphere of the cerebrum was perfectly healthy. The lateral ventricles were not congested, and contained but little serum. The space around the infundibulum was filled with a clot of blood. The cerebellum was in a normal condition. The posterior part of the medulla spinalis was congested. The basilar artery contained no clots of blood. All the portions of the brain not mentioned were in a healthy state.

Chest.—The left lung was slightly adherent to the walls of the chest, the right entirely free; both were somewhat congested, but contained no traces of tubercle or evidences of serious affection. The heart was of normal dimensions, somewhat loaded externally with fat, and softened in its tissues, but the microscope gave no evidences of fatty degeneration. The mitral valve was covered with small red vegetations upon its free margin; the other valves were healthy. The pulmonary artery was thinner and softer in texture than natural; the aorta was healthy.

Abdomen.—The liver was found to be highly congested, so as to be coloured nearly black, and so much softened as to be readily torn by the fingers. The spleen was also congested, marbled externally, but very little larger than natural, and the Malpighian corpuscles were found to be large and well defined. The left kidney was congested, and presented a cicatrix upon its free edge, probably the remains of an old cyst; the right was also congested, and presented three similar cicatrices. The left capsula renalis was healthy, the right was distended with fluid blood.

Pelvis.—The uterus was retroflexed, which may have been post-mortem, as there were no symptoms to indicate such a condition during life, and the neck was very soft and flexible, the broad ligaments relaxed, and the body of the uterus heavy from congestion. Upon opening the uterus by a vertical incision, its walls were found thickened, its cavity enlarged, the lining membrane dark-coloured from congestion affecting the entire organ, and within the body a small whitish object bearing some resemblance to an embryo of two or three weeks old. The appearance of the uterus, taken in connection with the fact that an oval fleshy body was discharged, makes it probable that she may have miscarried the day previous to her death. Whether the retroflexion of the womb was the cause of the abortion, was produced by the fall, or resulted post-mortem from the causes before mentioned, I am not prepared to decide. The extreme flexibility of the cervix would indicate that the retroflexion was of recent date. The ovaries were firm, white, and filled with vesicles. The mesenteric glands were slightly enlarged. The stomach and intestines were not examined, except as to their external appearance, which was natural.

The points most worthy of note in this case are: the comparative youth of the patient; the absence of the apoplectic diathesis; the previous debility, and its causes; the tendency of plethora capitis, notwithstanding the existence of anæmia; and the non-appearance of hemiplegia or paralysis in any form, although the amount of blood effused was large, and the variety of apoplexy cerebral.

Ring of Bone in the Axilla.—Dr. HARLAN exhibited a specimen of this, and stated that it was obtained from a subject in the anatomical rooms of the University. It is a distinct, firm bony ring, including the axillary artery, with the median, ulnar, and musculo-spiral nerves. The accompanying drawing, by Dr. Packard, taken after the specimen was removed from the body, gives the exact size and shape of the ring, Fig. 1, and shows the parts included within it, Fig. 2. The history of the case is not known. The

subject was a negro, apparently of middle age. There was no adventitious deposit in any other part of the body. The ring was found in the middle of

Fig. 1.

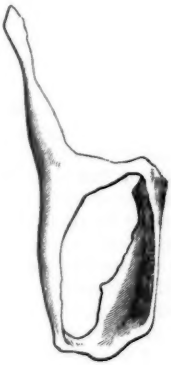


Fig. 2.

the right axilla. It had no connection with the humerus or with the sheath of the artery, but is connected by its posterior surface with the sheath of the musculo-spiral nerve. The limb was well developed, and showed no signs of any interference with its function.

Curious bony deposits are known to occur occasionally in healthy tissues as well as in morbid products, and pieces of true bone of considerable size have been found imbedded in muscle (in the museum at Vienna there is a large egg-shaped piece of bone, which was taken out of the biceps of a woman's left arm), but I have not been able to find any record of a case at all like this. Its shape and position are very peculiar; and it is singular, too, that the pressure to which every motion of the arm must have subjected the nerves has had no apparent effect upon the limb.

Two Cases of Yellow Fever; one sporadic, the other imported.—Dr. LA ROCHE made the following remarks relative to these cases:—

My friend, Dr. Packard, is about to present to the Society the results of

the post-mortem examination of two cases of yellow fever, which my official connection with the Board of Health afforded me the opportunity to notice —the first after death, the second during life. These examinations were made at my request, by Dr. Packard—the first with the assistance of our secretary, Dr. Kane. The history of both cases presents features of an interesting character; while, as you will perceive, the appearances revealed on dissection were sufficiently important to justify their being brought to the attention of the Society, and, indeed, of the profession at large.

The first case occurred in the practice of Dr. A. C. Bournonville, of this city, by whom it was reported to the Board of Health. It was a truly sporadic case (the disease not showing itself in a single other instance during the entire season), and, without a possibility of doubt, it must be regarded as of strictly local origin. The subject of it, a German, resided in Front Street below Coates, and worked in a board-yard situated at the foot of Green Street, on the Delaware front of the city. Some time previous to his attack he was employed on a raft conveying timber from the upper part of the river. He had not communicated, directly or indirectly, with any infected vessel from yellow fever ports; indeed, there was no such vessel to communicate with. He had not been within many hundred miles of a yellow fever case, from which he could, supposing the thing possible, have taken the disease; and had not been in any way exposed to the supposed morbific influence of fomites, whether in the form of merchandise or clothing. It may not be uninteresting to remark, in this connection, that at no great distance from the residence and place of work of the individual in question two sporadic cases (sporadic so far as regards that particular part of the city) occurred in 1853, and three or four a few years after.

The patient was visited by several of the medical members of the Board of Health, who are perfectly conversant with the yellow fever, and from their report of the cases; from the account of the symptoms, day by day, for which I am indebted to the attending physician, Dr. Bournonville; as also from the character of the matters ejected from the stomach and passed off by the bowels, no doubt can be entertained relative to the yellow fever nature of the disease. Indeed, could such a doubt have crossed the mind of any one during the progress of the case, it must necessarily have been dissipated by a view of the body after death, and especially by the inspection of the organs, the implication of which is admitted to characterize the said disease.

The patient was attacked early on the morning of Saturday, September 1, with a chill, followed by high fever, attended with severe pain in the back, arms, legs, and head. These symptoms, together with delirium at night, thirst, grayish diarrhœa, and epigastric pain, continued, with greater or less severity, till Tuesday, when the pulse fell to sixty, and presented some intermittence; the surface of the body became cold, and vomiting set in. The next day the conjunctiva became yellow, petechial spots supervened, and suppression of urine soon after attracted attention. The jaundice from this time gradually spread over the body, and increased in intensity. To these were added, on Friday, black vomit (some ounces of which were sent by Dr. Bournonville to the Board of Health), and, successively, black, tar-like stools, vomiting of unchanged blood, hemorrhage from the gums, and other symptoms equally characteristic. I must, while on the subject of symptoms, call your attention to the circumstance that in this patient the suppression of urine, which, as already stated, occurred on the third day, did not, as is usually observed in fatal cases, continue to the close of life.

Dr. B. writes on the fourth day that the patient "has not passed water for two days; no indication of the presence of urine in the bladder." On the fifth the doctor says: "Has not passed his urine; no discomfort on that account." At the close of the same day the doctor passed a catheter, and drew off two tablespoonfuls of thick, oil-like urine. The next day he again resorted to the catheter, and removed a wineglassful of yellowish-brown urine. On the eighth day about a gill of dark yellow urine, more limpid than the former portion, was removed. The next day, Sunday (ninth day), in the afternoon, Dr. B. "drew off 1¼ pint of thin, dark urine." The same quantity was drawn off the next day; and, as we shall see presently, the bladder was found, on dissection, to contain about a gill of fluid.

The case lingered till 11 o'clock P. M. on Monday, the tenth day, when he died at the City Hospital, whither he had been removed a short time previous, in order to allay the excitement produced in the vicinity of his residence by his presence.

The second case occurred in the person of one of the crew of the U. S. steamer Water-Witch, which arrived at the Lazaretto on the last day of September, from the south side of the island of Cuba. During the passage the Water-Witch lost two men with decided and unmistakable yellow fever, and, on reaching the quarantine station, landed five cases—four with a mild but well-marked form of the disease, while the other, the subject of the present remarks, who at his admission in the hospital had reached the fifth day of the attack, exhibited a combination of symptoms of a most formidable character. The appearance of the disease in the above-mentioned vessel presents some points of interest to which I must be allowed to invite your attention for a few moments. The Water-Witch lay for some time at anchor at the distance of about a mile from the port of Cienfuegos. While there, boats were sent to the town morning and evening each day—in the morning for provisions, and in the evening to convey the officers. No yellow fever prevailed in the place or among the shipping, which, unlike the Water-Witch, were anchored at the wharves, and consequently close to the town. No sickness occurred on board of the latter vessel, except a few cases of ephemeral fevers (*calenturas*), presenting none of the characteristic symptoms of, and in no way to be confounded with, yellow fever.

The vessel next went to the Isle of Pines, the healthiest spot in all Cuba, and where, so far as I can learn, yellow fever seldom, if ever, originates. There, however, the disease broke out in the vessel, and, after the occurrence of several cases, the captain determined to bring his vessel to the United States, in order thereby to arrest the further progress of the infection. Now, it was matter of observation that, with one solitary exception, none of the men attacked, whether at the Isle of Pines or on the passage home, had been ashore at Cienfuegos, or had manned the boats that plied from the vessel to that place mornings and evenings. Hence, even had the disease prevailed in the town or among the shipping—which, as we have seen, was not the case—those that sickened could not have taken it there. The exceptional case was not the first to suffer, and had so mild an attack as to leave a strong doubt as to the disease being the true yellow fever. I may mention that it is equally certain that though the first cases occurred at the Isle of Pines, they could not, for the reason stated, have originated from a poison derived from that place. My opinion is, and I give it for what it is worth, that the poison originated on board. One thing is certain, that all the cases were traced to the same part of the vessel—the fore division of the main deck, in the vicinity of the galley. This part is small—

much, very much too small for the number of men confined in it, considering, especially, the climate of the region where the vessel was cruising, and the additional heat issuing from the galley. It is, besides, low, badly ventilated, and near the pumps and hatches, from which issued a highly offensive smell, proceeding from the hold.

Of the symptoms noted during the progress of the cases in question I shall say little more than that they could not be referred to any other disease. They were so referred, not only by myself, but also by the medical officer of the vessel, Dr. Engles, by Dr. Filbert, of the Lazaretto, and by my excellent friend, Dr. Dickson, who saw the patient at my request, and whose familiarity with all the forms of yellow fever renders his opinion relative to its diagnosis of the utmost value. All I need add, in this connection, is, that the disease was protracted to the tenth day; that the jaundice was less marked than in the preceding case; that the black vomit was late in making its appearance, but at last assumed its usual characteristics; and that the suppression of urine, after continuing several days, ceased, a fair quantity of fluid being drawn several times a day by the catheter.

1. *Case of Sporadic Yellow Fever.* Examined at the City Hospital, September 11, 1860. Present, Drs. La Roche, Jewell, McCrea, Bell, Kane, and Bournonville.—Rigor mortis very well pronounced. Body yellow, with a good deal of congestion of posterior surface and extremities. A quantity of dark liquid was oozing from the nose, and a flow of yellow, frothy mucus, tinged with blood, took place when the body was moved.

The areolar tissue, subcutaneous, submucous, and subserous, was universally tinged deep yellow, and was remarkably firm. In the costal cartilages it was evident that this tinging was confined to the perichondrium.

Both *lungs* were healthy, but hypostatically congested; the pleuræ full of deep yellow serum. The *heart* was of good size, and its lining membrane stained yellow; its tissue was degenerated, the change being perhaps fatty, but with a granular appearance under the microscope. The pericardium was deeply tinged yellow, and contained about f℥j of very yellow serum; at one portion of its parietal layer there was a spot of well-marked extravasation. The left cavity of the heart contained a firm yellow clot; the right auricle was distended with a very large clot, of a dark, currant-jelly-like appearance. Both the aorta and the pulmonary artery were stained red.

The *stomach* contained a black or very dark brown grumous matter, and its mucous membrane presented here and there patches of extravasation. The *duodenum* seemed congested and softened, the rest of the intestine healthy. The *liver* had a decidedly fatty appearance, but did not exhibit to the full extent the oil-globules when examined microscopically; its colour was very light, and its tissue easily torn. The gall-bladder was distended with intensely black viscid bile. The portal vein was distended with fluid blood. The *spleen* had externally a greenish colour, from the yellow tinge of its capsule; its pulpy substance was very red and soft. The *kidneys* were enlarged and congested, the supra-renal capsules apparently healthy. Some enlarged glands were noticed around the pancreas.

2. *Case of Yellow Fever, from U. S. Steamer Water-Witch.* Examined at the Lazaretto, October 3, 1860, with Dr. Engles, U. S. N.—Body examined twenty-two hours after death; very muscular and well formed. It was not yet cold, although the rigor mortis was very well pronounced. The surface was somewhat tinged yellow, and there was slight ecchymosis in the posterior portions, in the ears, penis, and scrotum, not much in the

extremities. About the mouth there was a quantity of dark brownish foam. On lifting the body, and changing the position of the head, a copious flow of bright red blood, apparently arterial, took place from the nostrils.

Thorax.—Old pleuritic adhesions were found at the lower part of the right lung. Both *lungs* were healthy, but presented an intense degree of hypostatic congestion. The *pericardium* was distended with air, a circumstance I do not remember ever observing before, and which I cannot easily explain. The *heart* was healthy in appearance, but its texture seemed degenerated. A microscopic examination subsequently made, with Drs. Darrach and Da Costa, showed that it had undergone fatty change, as was the case also with the liver. Both the aorta and pulmonary artery were deeply stained red. The blood was everywhere entirely fluid.

Abdomen.—*Liver* fatty in appearance; gall-bladder excessively distended, and stained with intensely black viscid bile. *Stomach* deeply congested at its lesser curvature; towards its cardiac extremity its mucous membrane presented a dark greenish colour, with an appearance here and there of minute white vesicles. *Duodenum* slightly congested; *intestines* not decomposed at all, but congested at some places for a considerable distance; they were full of a dark, viscid, tarry matter; the solitary glands were enlarged, as were also the lymphatic glands of the mesentery. Peyer's patches were healthy. The *spleen* was rather large, intensely congested, and almost diffluent. The *kidneys* were flabby, but not otherwise abnormal. On the external surface of the supra-renal capsules there was noticed some injection of the vessels.

I shall add but a few words to what you have just heard. In regard to the condition of the liver, you will notice that in neither of the cases did the organ exhibit the fatty degeneration to the extent it generally does in fatal cases of the disease under consideration. Nevertheless, it was in both decidedly oily; at any rate, sufficiently so, in all respects, to justify us in adhering firmly to the opinion, now very generally entertained, that this condition of the liver ranks among the main, if it is not *the* most important, of the anatomical characters of the disease. I may mention that in both instances additional proofs were obtained of the correctness of the opinion that the change is due to a fatty and not to an amyloid degeneration, as suggested by Prof. Joseph Jones, of Georgia, and maintained, at one time at least, by Prof. S. Jackson, of this city. This fact, so far as I can ascertain, was first experimentally demonstrated by our fellow-member, Dr. Da Costa, in a case from the West Indies, which was admitted, under his care, in the Episcopal Hospital some two years ago (August, 1858). In this instance, as in the two before mentioned, the oily matter was partially dissolved by ether, and not acted upon by iodine or sulphuric acid.

In both cases before us this evening the heart was found granular. This condition of the heart in fatal cases of yellow fever was, so far as I can ascertain, first pointed out by Prof. Riddell, of New Orleans. By him it is looked upon as the result of the molecular disarrangement of the muscular fibres of the organ, in virtue of which those fibres lose their natural structure, and assume the non-striated or granular aspect. "In general," as he remarks, "the molecular change is either complete or well marked, rarely it is very slight or imperceptible." "The molecular degeneration of the heart, unlike the fatty change of the liver, can always be satisfactorily made out, affirmatively or negatively, with a good microscope. It is on this account, perhaps, that in fatal cases of yellow fever the molecular change in the heart

has appeared to me to be a lesion much more constant than fatty degeneration of the liver."

Whatever may be the fact relative to the frequency of the change in question, I very much doubt the correctness of the conclusion at which Prof. Riddell has arrived, that it is more constant in the cases mentioned than fatty degeneration of the liver, inasmuch as I did not fail to discover this last lesion in the many autopsies I witnessed in 1853 and 1854, and some I have had occasion to witness since. I know, besides, that the same result has been obtained by pathologists in other places in this country and abroad, and more especially, and on a large scale, in Lisbon, during the severe epidemic by which that city was visited in 1857. Dr. Lyons, the able historian of that epidemic, does not use on the subject language calculated to make us conclude that the change was there only of occasional occurrence, or yielded to any other in point of frequency. His words are worth recording. "The most remarkable, the most constant, and, to my mind—I will frankly avow it—the most inexplicable condition presented in the *post-mortem* examination of fatal yellow fever cases, was the state of the liver. I believe it may be affirmed that *some departure from the normal state of this organ was an absolute condition in all cases which proved fatal.* It was not only so with regard to the cases examined by myself, but the concurrent testimony of all my learned confrères in Lisbon pointed to the same result." "The fawn-yellow coloration usually well indicated the change that had taken place in the hepatic tissue; but it was not only in those cases in which the liver presented this coloration that abnormal states of the hepatic structure existed. The yellowish-brown colour—'chocolat au lait'—was attended with similar and fully as well-marked changes. The same may be said of the nutmeg condition of the organ, and even, to some extent, of several cases in which the ordinary liver-brown colour was unchanged."

"Minute fine sections, or matter scraped from the hepatic texture, exhibited the hepatic cells filled with globular oily and fatty matter. The natural appearance of the cell was completely altered, its outline obscured, and its nucleus rendered invisible. It was surcharged with molecular and globular oily matter, while the whole field and the interspaces between the cells were filled with similar and equally abundant oily and fatty elements, were in fact dissipated, and the contour of the cells brought more clearly into view; but it was only rarely that the nucleus could, by long treatment in ether, be made visible."

In view of what precedes, I cannot conceive how the above-mentioned change of texture of the heart can justly be said to be much more constant in fatal cases of yellow fever than the fatty degeneration of the liver. How a particular pathological condition of a given organ can present itself more constantly in the aforesaid disease than the lesion of another organ which is always found on the dissection of subjects who have died of the same disease, is a puzzle, the solution of which I leave to others. But while unwilling, from all I have seen myself or learned from others, to admit that the change in the liver is less frequently noticed in fatal cases of yellow fever than the above-mentioned granular condition of the heart, I cannot but be disposed to regard the latter, I will not say as a pathological change of constant occurrence in such cases—in other words, in the light of an anatomical character of the disease—but certainly as one very frequently noticed, and which deserves serious consideration in a pathologico-diagnostic point of view.

I lay no stress on the circumstance that Dr. Lyons, though describing the condition in which he found the heart in his dissections at Lisbon, says nothing of the one under present consideration, inasmuch as, from his silence on the subject, we cannot conclude that it did not exist. At least such is my belief, founded on the fact that we are nowhere told by that pathologist that he examined the substance of the heart in the only way in which the granular deposits can well be discovered—namely, under the microscope. Be this, however, as it may, this particular morbid change was noticed by Dr. Da Costa in the case already mentioned as having occurred in the Episcopal Hospital. I hasten to state that Dr. Da Costa was not at the time aware of Prof. Riddell's observations on the subject. Dr. D. was kind enough, knowing the interest I take in all matters connected with the yellow fever, to show me the specimen in his possession. I have since seen the same change in another specimen exhibited to me by a friend. Dr. Da Costa, I am told, has also again seen it; and now we find it in both of the cases to which your attention has been invited this evening.

In conclusion I have to state that Dr. Riddell says nothing of the nature or composition of the aforesaid granules. To Dr. Da Costa credit is doubtless due for having decided the question. In the Episcopal Hospital case the heart was found soft and flabby; the muscular fibres were exceedingly granular; here and there oil-drops were seen; the striæ of the muscle were observed; and the granules were to a great extent soluble in ether, but not in acetic acid. I was fortunate enough to witness this examination and experiment, and, were it necessary, which we all know is not the case, could vouch for the truthfulness of the above statement. The same result was obtained in both instances before us. The conclusion from this is that the granules are fatty, the consequence of a commencement of fatty degeneration. I have no hesitation in claiming for Dr. Da Costa credit for the discovery of that fact.

Nov. 28. *Medullary Cancer of the Liver.*—Dr. CHARLES C. LEE presented a specimen of this, with the following observations: The subject of this case was a girl, 22 years of age, born in Glasgow, Scotland. Her father died four years ago, in his sixty-seventh year, of *cancer of the breast*, as she was told by the physician who attended him; her mother and the rest of her family were healthy. The patient herself had no recollection of any ill health before her twelfth or fifteenth year, when she contracted a severe cough. This continued three years, when she came to Philadelphia for medical advice, and was admitted to the Pennsylvania Hospital in June, 1859. Here she remained several months, recovering in some degree from her cough, which, however, returned after she left the hospital, and in the beginning of last January she entered the medical wards at Blockley for the same trouble. In a fortnight she was much better, and was discharged to the out-ward, or almshouse proper; but on the 5th of May she was sent back, with a severe attack of jaundice, accompanied by sharp, lancinating pains in the hepatic region. From that time until her death, on the 24th of November, menstruation was completely arrested, and her health generally became deranged. Although subjected to various plans of treatment by Drs. Da Costa, Judson, and Ludlow, who successively had charge of the case, the liver advanced steadily in size, gradually extending above the ensiform cartilage, and downwards to within a quarter of an inch of the umbilicus, behind to the spinal column, and deeply into the left hypochondriac region in front. Over this space the skin was exceedingly tense; the

whole body was deeply jaundiced and emaciated, except the lower extremities, which remained œdematous until the last.

Autopsy, thirty-six hours after death.—Body œdematous, but not equably so; right arm and side generally much fuller than the left, particularly the upper extremity. Eyes and skin of entire body deeply injected with bile; no rigor mortis observable. On opening the abdomen, the liver was found immensely enlarged, extending on the left side three inches, and on the right four inches below the ribs; upwards to between the second and third ribs. The diaphragm was adherent at several points. The right lobe was much more enlarged, proportionally, than the left, and was also soft and fluctuating. The entire weight was 15½ pounds. When the right lobe was incised, about a quart of thick, dark brown liquid was discharged, consisting, apparently, of broken-down liver-structure, for no pus was perceptible; floating in this liquid were seen, here and there, small masses of whitish pulp. A portion of this fluid was submitted to microscopic examination by Dr. Woodward, whose report is as follows: "The whitish pulp is composed of large spindle-shaped cells in great abundance, a few free nuclei, and large oval cells with single and plural nuclei and nucleoli. A mass of this character imbedded in the substance of the liver would be *medullary cancer* of the liver, and such cancer masses often soften centrally so as deceptively to resemble an abscess, if studied only with the naked eye. They may be single, or several may exist in the same liver, with or without cancer elsewhere." The piece of liver tissue sent to Dr. Woodward was fatty, and so altered as to be scarcely recognizable. In the peritoneum was found about a quart and a half of serous effusion, strongly tinged with bile. The heart was normal; lungs sound, but compressed, especially on the right side. The kidneys and spleen were natural.

Case of Mola Hydatidosa.—Dr. KELLER reported this case as follows: Mrs. E., 32 years of age, of healthy constitution, the mother of five children, had her menses last on the 6th of January. She felt otherwise quite well until the 18th of November, when, at 5 o'clock P. M., she was taken with regular labour pains. Four hours afterwards the mass I now exhibit was expelled. She lost but little blood, and the os tincæ contracted readily.

The tumour weighed not quite four ounces, was of an ovoid form, 1½ inch wide, and 2½ inches long. On one side it showed a smooth surface, with no appearance of direct vascular communication with the uterus. On the other side there was a perforated empty sac, formed by a thin membrane, covering the undulating surface of the rest of the tumour. In opening that part of the mole next to the womb, it presented a perfect framework of white fibres and cyst-like formations. The fibres were in many instances straight, in others of the rosary-like appearance. They consisted, under the microscope, of connective tissue, covered in many spots with the characteristic villi of the chorion. Some of these were filled with regular cells, others contained only small molecules. In other instances the fibres of the connective tissue ended in cell-formations. The smaller nodules in connection with the fibres were solid, and consisted simply of villi. The larger ones, of the size of small beans, were connected to the framework by minute filaments, and showed on the surface capillaries. I did not examine them, thinking to be able to preserve their characteristic appearance until this meeting of the Society. Next day, however, I found them entirely collapsed. Between this formation and the membrane before mentioned there was an elastic mass, reddish on section, and looking like a fresh fibrinous

clot. Under the microscope this was seen to consist of unorganized granular matter.

Aside from different isolated cell-formations, this mole consisted of—1st, connective tissue; 2d, villi belonging to the chorion; 3d, unorganized fibrin; 4th, the membranous sac covering the free surface.

It may be added that Prof. Virchow has found in the distended villi of a fresh "mola hydatidosa" a substance which he calls *mucous tissue*, which he considers as distinguishable from fully-formed connective tissue and colloid matter by its reaction with acetic acid.[1] He has found it also in the umbilical cord, vitreous body, and in many pathological products which have been classed with colloid growths.

Dr. KEATING spoke at considerable length on the subject of uterine moles. His experience led him to believe that they were invariably the result of blighted ova; fatty degeneration then ensued; and they might either be discharged by a slow oozing of an oily substance from the womb, or in a mass; so altered, however, as to give but little evidence of their origin. From the history of the cases which he had met with, he believed that the death of the fœtus almost invariably preceded the formation of the hydatiginous mole. The effect of prolonged lactation during pregnancy was instanced, as causing slow and persistent contractions in the uterus, sometimes producing inflammation of the placenta, and consequently death of the fœtus from obstruction of the placental circulation; oftener, however, the death of the fœtus ensuing as a simple result of the continuous uterine contractions detaching or constringing portions of the placenta, and thus involving the death of the fœtus. In two instances which had come recently under his observation the ova were so altered as to present scarcely a trace of their original organization. It was hardly necessary to call attention to the fact that the hydatiginous character of these moles is entirely due to the villi of the chorion, which do not cease developing upon the destruction of the ovum. In one lady under Dr. K.'s care, who had discharged, in the course of four years, three of these hydatiginous moles, he had detected a latent constitutional poison. A continued appropriate treatment was followed by the birth of a healthy child at full term. The three hydatiginous masses just referred to gave evidence, on examination, of placental inflammation, probably the result of the action of the constitutional virus. Dr. K. begged to call the attention of the Society to the fact that constant and violent retching in the early months of pregnancy might also induce slow contractions in the uterus, destroy the ovum, and give rise to formation of hydatiginous moles. He then dwelt upon the frequency with which hemorrhages from the uterus, caused by the *débris* of portions of the fœtus remaining in the uterus after abortion, were mistaken by practitioners for repeated miscarriages. He cited, among others, an instance of a lady in whose case he had been recently called in consultation on account of repeated abortions, or floodings from threatening abortion, as supposed by her medical attendant. She had been kept several months in the horizontal position, and had had several applications of the nitrate of silver to the os and cervix uteri, for supposed ulceration. The history of the case led him to suspect the existence of some foreign body within the cavity of the uterus. An exploration with Simpson's sound having convinced him of the correctness

[1] Mucous tissue containing *mucin*, which is precipitated by acetic acid, and insoluble in an excess of it; connective tissue and colloid matter being affected, if at all, in the way of solution.—*Sec. Path. Soc.*

of his suspicions, he succeeded in extracting a mass of the size of a large walnut, flattened, and consisting of concentrated laminæ of a fatty substance, being undoubtedly the remnants of the placenta of the last aborted ovum. Upon its extraction the floodings ceased; the lady entirely recovered her health, is again pregnant, and has nearly reached the full term. The occurrence of four such cases in Dr. K.'s practice induces him to believe that they are much more common than is generally thought. It behooves the obstetrician, therefore, always to insist upon examining the matters discharged from the uterus; and in cases of doubt, and especially where the hemorrhage from the uterus, as in the cases just referred to, is prolonged, or is constantly recurring, it is incumbent upon him to make such an examination of the uterus as to place all such cases beyond a doubt. In conclusion, he firmly believed that all uterine moles were invariably the results of impregnation, the clots and membranous discharges from the unimpregnated uterus having a distinct character and history.

Dec. 12. *Chronic Alcoholism.*—Dr. HALL exhibited a heart, the muscular tissue of which was in a state of marked fatty degeneration. The patient was a tall, strongly-built, married female, about 55 years of age, but rather younger in appearance. For nine or ten years past she had been an habitual but secret dram-drinker. Although hardly ever drunk, she could scarcely ever be called perfectly sober.

On the 24th of November she first came under notice, having complained, however, for two weeks previously of pains in her limbs. Before this, although unable to go out of doors, she could go about her house, but now she became so weak as to be obliged to keep her bed. She complained of total loss of appetite, and of sleeplessness; her pulse was 96, and soft; her tongue coated. She had no headache, and her vision was not affected; she had no delirium, no tremor, no pain in the chest or abdomen. Her bowels were constipated. She was quite helpless, and unable to sit or stand without support. The vacant, abstracted manner of this patient would strike the observer as peculiar. Apparently taking no notice of surrounding objects, she answered correctly, though slowly, when spoken to. Although doing so, there was a certain amount of hesitation or difficulty of articulation, as though there was a want of co-ordination between the different organs of speech, exceedingly like a symptom apt to occur in the progressive paralysis of the insane.[1]

This, in fact, was the cause of alarm to the family, and led them to call in a physician. At 5 o'clock the previous afternoon she appeared unable to answer a question put to her, although her lips moved in the effort to do so. The affection of the motor and sensory apparatus was peculiar. There was almost entire loss of sensibility in the left leg up to the knee, even when pinched, whilst the sensibility of the thigh appeared to be much increased. This was also the case, to a less extent, in the arm of the same side. In the cases reported by Dr. Huss there was a constant restlessness of the limbs observed, moving them up and down in the bed. Here it was only observed in one of the arms—the right—which was constantly raised up and let down again by the patient. The appearances present were such as would most naturally and readily be referred to the nervous system. Their gravity, however, was not to be accounted for by the actual

[1] On the Progressive Paralysis of the Insane. By Wm. Wood. Brit. and For. Med.-Chir. Rev., July, 1860, p. 187.

amount of disease, and would have rendered the diagnosis a difficult one, had it not been for the elaborate description by Dr. Huss of a disease prevailing among excessive spirit-drinkers in Sweden.[1]

Under moderate stimulation some little improvement took place, but it was not of a permanent character. She had a slight convulsion on the morning of December 1, which, however, did not seem to have any effect upon the general symptoms. This condition of things continued a week longer, when she became comatose, but without stertor; the sphincters were relaxed, and the left arm completely paralyzed. The pupils, though contracted, were mobile; and upon pinching the unaffected limb, she was able to draw it up. She died very quietly on the morning of the 10th inst.

Autopsy, thirty-seven hours after death.—Head not examined, the family having refused permission.

Chest.—The lungs presented nothing of importance. The mediastinum was loaded with fat; the pericardium normal in colour and texture, but distended with serum to the amount of six ounces. The *heart* was on its external surface loaded with fat, which also appeared to have encroached upon the proper muscular tissue of the walls. The organ appeared flabby, dilated, and attenuated, the left ventricle more particularly. The valves were in normal condition. The *aorta* was quite healthy.

Abdomen.—The omentum was loaded with fat, extending almost to the pubis. There was an excessive deposit of fat throughout the abdominal cavity. The *stomach* was, in its pyloric half, much reddened, and slightly mammillated; the redness was not altered by washing. Otherwise the mucous membrane appeared to be in a healthy condition, without any evident thickening or softening. The *liver* was partially enlarged, the left lobe equalling the right in size. The entire organ was pallid, and in a state of moderate fatty degeneration. Gall-bladder distended. The *kidneys* were pale, and anæmic-looking; upon the upper surface of the left there was a small cyst.

The above case presents so many points of interest, both to the pathologist and physiologist, and so closely resembles in its symptoms the descriptions of Huss, that it was thought not unworthy of contribution to the literature of the subject.

1861. *Jan.* 9. *Intestinal Concretion in the Appendix Cæci, causing Perforation and fatal Peritonitis.*—Dr. PACKARD presented this specimen. The patient had been under the care of Dr. J. F. Meigs, who supplied the ante-mortem history of the case.

T. D. S., a healthy, well-grown boy, 11 years of age, rose on the morning of December 25, 1860, apparently quite well. Soon afterwards, however, he complained of pain in the right iliac and lumbar regions, was chilly, and returned to bed. A dose of castor oil was given him. In the course of the day fever came on.

Next day Dr. M. was called, and found him feverish, with a pulse of 132, a hot and dry skin, and a moderately furred tongue. The pain still continued, with tenderness and slight distension of the abdomen on the right side; there was no vomiting. His bowels had been acted upon three times by the oil. Leeches and a poultice locally, and a mixture of blue pill with rhubarb syrup internally, were ordered.

[1] M. Huss, Chronische Alkoholskrankheit, oder Alkoholismus Chronicus. Aus dem Schwed. von Gerh. v. d. Busch. Leipzig, 1852. p. 8.

On the 27th and 28th the symptoms were much the same, except that the tenderness and distension increased. The pain was aggravated by coughing, by a full inspiration, and by motion, especially of the right leg. Bowels slightly moved by the mixture; no vomiting as yet. His fever continued, but his pulse fell to 108, and his skin was somewhat cooler.

On the 29th he was worse. All his symptoms were aggravated, and vomiting set in; his bowels became confined. Small doses of calomel and opium were given, enemata of various kinds were tried, and rhubarb syrup with a little fluid extract of rhubarb was perseveringly employed, but without effect.

The abdomen now became greatly distended, exceedingly sonorous, and painful; the stomach grew more and more irritable, rejecting from time to time, towards the last, with a sudden, spasmodic effort, everything that was taken by the mouth. The bowels were completely obstructed, so that repeated injections of various kinds elicited no discharges, even of flatus. The urine continued to be secreted to the last; and there was at times, in spite of the nausea and vomiting, quite a strong desire for milk and bread.

During the last few days wine-whey and beef-tea were given in small quantities; and opium by enema and by the mouth was used to allay pain. On the third day of the treatment a blister four inches square was applied over the seat of tenderness; but neither this nor any of the other remedies employed seemed to exert the least effect upon the course of the disease.

Death took place on the 1st of January, 1861.

The *autopsy* was made by Dr. Packard, twenty-four hours after death.

Body large, muscular, and well formed; rigor mortis well pronounced. Abdomen only examined.

On making the usual section, several coils of small intestine, very greatly distended with gas, and markedly injected, with flakes of lymph here and there over the surface, at some points gluing the adjacent coils together, were seen concealing the rest of the abdominal viscera. After some search, the colon was found, very much contracted, except at the cæcum. The ileum was in like manner contracted, the narrowing beginning at about the end of the jejunum, which formed the distended coils above mentioned. No cause was assignable for the constriction at this point, but a little lymph was thrown out here, and it may have been that the bowel had been twisted.

The appendix vermiformis was bound down by peritoneal adhesions; within it, near its root, was a mass as large as a small bean, but perfectly oval. Just beyond this mass, at what seemed to have been its position, was an ulcer, extending all round the tube, and of a gangrenous aspect. At the distal end of this ulcer was a perforation, by which matter had found an exit into the peritoneal cavity. The rest of the tube looked as if it had been distended by the pus before the opening was formed.

After its escape from the appendix, the matter seemed to have caused a circumscribed peritonitis, in addition to the general one already indicated. The adhesions bounding this peritonitis had extended up to the liver, the convex surface of which was hollowed to a slight depth in an oval shape, the depression being lined by false membrane. The whole quantity of the pus was perhaps f℥iv.

The liver was pale in patches, but was not degenerated. Rather a larger number of oil-drops existed in a dark, inflamed portion of its substance just beneath the depression above mentioned, but even here the quantity was not great.

Mesenteric glands swollen, and injected over the surface.

No other lesions were observed.

Jan. 23. *Serous Cysts of the Breast.*—Dr. H. LENOX HODGE, in presenting this specimen, said: This breast was removed by Dr. Norris from a lady about 35 years of age. She first noticed a tumour in her right breast some seven years ago. It did not appear to increase much in size until last fall. It then began to enlarge, and at the time of the operation presented the appearance of an irregular tumour about two inches in length and breadth. Both breasts were small; the lady had been married for four years, but had never been pregnant. The whole gland was removed, as other cysts were probably scattered throughout it. During the operation the largest cyst was opened. It contained an ounce or two of thin serous fluid. There were found to be some ten cysts, all apparently situated in the midst of the tissue of the gland. They varied much in size, the smallest being about as large as the little beads of children's toys. They were provided with a distinct membrane, and could be readily turned out entire from their bed of fibrous tissue. The contents of most of them consisted of a light-coloured, thin serum; but in a few the fluid was of a deeper colour, and much more glutinous. The microscopical examination of the cysts, their contents, and the surrounding tissue, has been made by Dr. Woodward.

Dr. WOODWARD said that simple serous cysts of the mammary gland were so rare, that he had examined the present specimen with considerable interest. Cysts associated with neophytes of connective tissue (cysto-sarcoma), or with cancer (cysto-carcinoma), were far more common. A portion of the specimen before the Society had been presented to him by Dr. Hodge, and he had examined it, with the following results:—

Imbedded in the morsel of gland-tissue was what at first appeared to be a single cyst, but which more exact observation showed to be three cysts; the largest about the size of a small cherry, and the smallest that of a pea. They were in the closest juxtaposition, and only separated by the thinnest septa.

On puncturing the largest of these cysts, a *clear yellowish fluid* escaped, which became slightly turbid on boiling, showing the presence of a *very small* quantity of albumen. Repeated examinations showed no forms in this liquid, except a few minute oil-globules. The *walls* of this cyst were vascular, and composed chiefly of well-developed white fibrous tissue, in which were imbedded a considerable number of more or less atrophied fat-cells, in which the *nucleus* and a number of drops of diverse size, and composed of high-coloured fat, could be distinguished readily.

The fluid of the second cyst was more viscid, coagulated more abundantly, and therefore contained more albumen than the first. It also contained large numbers of large, pale cells, with delicate contours and dimly granular contents, $\frac{1}{850}$ to $\frac{1}{800}$ of an inch in diameter, and each containing a clear nucleus of from $\frac{1}{3300}$ to $\frac{1}{2500}$ of an inch in diameter. Some of these cells were partly destroyed, and the nucleus set free, with or without a portion of the contents adhering to it. They were sometimes adherent by their edges, and were evidently derived from *the distinct pavement epithelium* with which the cyst was lined. This consisted of tolerably regular hexagonal cells, rather smaller and more granular than the bloated ones floating in the fluid—viz., $\frac{1}{1200}$ to $\frac{1}{1000}$ of an inch in diameter, with nuclei $\frac{1}{6000}$ to $\frac{1}{3000}$ of an inch. The epithelium reposed on a fibrous layer, constituting the wall of the cyst, and, like that of the first, vascular, and containing atrophying fat-cells. The third cyst was not examined microscopically. The tissue of the mammary gland was, in every respect, perfectly normal, so far as examined; the gland-vesicles presenting the

usual characteristics. Groups of gland-vesicles, with the accompanying milk-ducts, also perfectly healthy, were imbedded in the outer layers of the walls of the cyst.

With regard to the origin of mammary cysts in general, Dr. W. expressed the opinion that any exclusive doctrine on this subject must inevitably fall to the ground in the presence of individual cases. Mammary cysts may undoubtedly arise by the distension of one or more of the ultimate gland-lobules; perhaps even a lactiferous duct may dilate into a cyst in consequence of obstruction. But besides these methods, by which the gland itself might contribute to cyst-formation, cysts may arise in the areolar tissue connecting the lobules together, by a dilatation of one or more of the areolæ. Rokitansky's idea, that cysts are special heterologous products, which begin as microscopic nuclei, and dilate into cysts, had not met with general credence.

Besides the theories of cyst-development with which he was familiar, one had suggested itself to him which he considered worthy of mention, and that was the possibility of the formation of cysts, by a process of *vacuolation*, in an intercellular matrix, similar to that by which the areolæ of areolar tissue, the intercellular passages in leaves, and perhaps all the closed cavities of the body, are developed.

He had seen in the villi of the chorion, in a case of so-called "hydatid mole," microscopic cysts which appeared to be merely cavities formed by the solution of the jelly-like intercellular tissue, pushing the cells aside, and limited apparently by no distinct wall, other than the substance in which they were excavated.

In the mammary cysts above described, he believed, the presence of the atrophying fat-cells in the walls was sufficient to show that the tissue of the cyst-wall was not of new formation, but represented the normal connective tissue of the gland, pushed aside, and distended by the accumulation of the liquid. The cyst described as lined by epithelium he was disposed to regard as having originated in the dilatation of one or more of the gland-lobules; the epithelium being furnished by the overgrowth of the secreting cells of the lobule. The first cyst, which was larger, might represent one of similar origin in which the epithelium had perished; or perhaps it had originated in the connective tissue between the lobules, and not in these themselves.

FISKE FUND PRIZE ESSAY.

ART. XII.—*On "The Morbid Effects of the Retention in the Blood of the Elements of the Urinary Secretion."* By WILLIAM W. MORLAND, M. D. (With two woodcuts.) The Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860.[1] (Published by request of the Rhode Island Medical Society.)

No organs in the human body play a more important part in the economy of life and health than the kidneys—their office is *the depuration of the blood.* In however slight a degree their function is interfered with, some untoward effects are produced. These may often be barely noticed, and easily recovered from; in many instances, however, although disregarded at first, they are sure of their ground, hard to be dislodged, and too frequently insidious and widely and surely destructive. The more open and overwhelming attacks of disease, which, by rapidly disabling the kidneys, and extensively injuring their tissue, at once and distinctly tell upon the constitution, reveal in plain characters the close connection between the vital torrent and its purifying agents.

The subject, as proposed by the Trustees of the Fiske Fund, necessitates, *first*, the enumeration of "the elements of the urinary secretion;" and *secondly*, the recital of the effects produced by the undue "retention" of each of them in the blood.

By the expression "elements of the urinary secretion," as here used, we understand its constituents in a state of health. These constituents, by a vital law, are to be eliminated from the blood; and their retention therein, beyond a certain time, will certainly cause "morbid effects."

The following enumeration of the urinary elements is taken from one of the latest and most reliable authorities.[2] The analysis is made up from an average of the composition of all the urine passed in twenty-four hours. *Average quantity* from twenty-four hours, 1400 to 1600 cubic centimetres;

[1] The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held in Newport, July 11, 1860, announced that the premium of one hundred dollars offered by them on the following subject: "The morbid effects of retention in the blood of the elements of the urinary secretion," had been awarded to the author of the dissertation bearing the motto—

"*Prius cognoscere, dein sanare.*"

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Wm. W. Morland, M. D., of Boston, Mass.

JAMES H. ELDRIDGE, M. D., East Greenwich,
CHARLES W. PARSONS, M. D., Providence,
HENRY E. TURNER, M. D., Newport,
Trustees.

S. AUG. ARNOLD, M. D., Providence, *Secretary of the Fiske Fund.*

[2] J. L. W. Thudichum, M. D., Lecturer on Chemistry at the Grosvenor Place School of Medicine, &c. "A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis." London, 1858.

49 to 56 fluidounces. *Average specific gravity*, 1.020. *Mean amount of solids*, 55 to 56 grammes (a gramme is 15.4440 grains, English).

Constituents.

Water	1345 to 1534 grammes.		
Urea	30 to 40 "	=	463 to 617 grs.
Uric Acid	0.5 "	=	7.5 "
Creatine	0.3 "	=	4.5 "
Creatinine	0.45		7.0 "
Sarkine, Uræmatine, Uroxanthine	Undetermined.		
Hippuric Acid	0.5 grammes.		7.5
Chlorine	6 to 8 "	=	92 to 123 "
(or Chloride of Sodium	10 to 13 "	=	154 to 200 ")
Sulphuric Acid	1.5 to 2.5 "	=	23 to 38 "
Phosphoric Acid	3.66 "	=	56 "
Potash and Soda, Lime and Magnesia	Undetermined.		
Earthy Phosphates	1.28 "	=	19 "
Iron	Undetermined.		
Ammonia	0.7 "	=	10 "
Trimethylamine, Carbonic Acid, Phenylic Acid, Damaluric Acid	Undetermined.		

"The minor estimates account for 48 out of 55 grammes of solids, the larger estimates for 62 out of 66 grammes of solids."—THUDICHUM, *op. cit.*

From an examination of the above table, in connection with the requisitions of the subject, it will be evident that we have only to indicate the pathological effects arising from the undue retention in the blood, of the following constituents of the urinary secretion: Water; Urea; Uric Acid; Creatine; Creatinine; Hippuric Acid; Chlorine; Chloride of Sodium; Sulphuric Acid; Phosphoric Acid; Earthy Phosphates, and Ammonia. The other ingredients of the urine, mentioned as being found in "undetermined" proportions, cannot enter into the list, in a practical consideration of the subject.

WATER.—Taking up the urinary constituents in succession, we first examine the results to be observed when that amount of *water* which should be excreted through the agency of the kidneys, is not so evacuated. This portion of our subject may be comprehensively disposed of.

A very variable amount of fluid is evacuated from the bladder at different seasons of the year, and under peculiar and differing circumstances. Thus, in cold weather, the amount of urine is greater, because the cutaneous transpiration is less. Again, when large amounts of liquids are ingested, somewhat corresponding quantities are excreted by the kidneys. The action of abnormally produced sugar occasions diabetes; certain medicines induce or augment, whilst others restrict, or nearly suspend, the urinary flow. Organic disease, or accidental obstruction, may cause almost complete cessation of urination; and entire *anuria*, although rare, occurs, from well-known causes.

Whatever, therefore, essentially diminishes, or actually suspends, for a longer or shorter time, the urinary evacuation, causes the retention in the blood of *all* the constituents of the urine, or of a goodly proportion of them. The deleterious effects consequent upon such a retention, will be

referable, in the main, to the presence of the solid constituents of the urine, rather than to that of an unusual supply of the watery vehicle. A certain amount—a redundance, even—of water, is absolutely necessary in the circulation, in order to eliminate, wash out, and bear on, as through a sewer, the effete, nitrogenous products, foreign to life, and incompatible with the integrity of the blood. And, besides this necessity for a surplus amount of water, it is rare that enough more than a normal amount is retained in the blood, to be of essential consequence, compared with the effects arising from the presence of the solids of the urinary secretion, prevented from issue by the same cause or causes which retain the watery portion. It is true, however, that "when urea is retained, water is also mostly retained in part, and, by its effusion into the cavities and cellular tissue, causes dropsical disease." (*Thudichum*, op. cit., p. 75.) But the action of the causes just alluded to is rarely or never sufficiently long maintained to be efficient in producing a deteriorated condition of the blood, *referable to excess of water alone*, the kidneys being healthy. Other morbid influences, arising from the presence of the solids of the urinary secretion, and the persistent action of the retaining cause upon the organs themselves, would produce far more rapid and appreciable effects upon the system at large, and upon the blood, than a simple increase of water, only, could do. It is acknowledged, however, that scanty urine—diminished both as to solid and fluid constituents—is indicative of a greater or less degree of anæmia. On the other hand, symptoms of hydruria may be favourable in certain diseased conditions—as where hydræmia and dropsy exist—and its actual establishment, either naturally or by artificial diuresis, may carry off the misplaced water, and restore the balance of the circulation. The profuse flow of watery urine in hysteria is often critical—at all events, *per se*, it indicates no blood-disease. Co-existent anæmia, in such cases, doubtless depends on some other cause than retention of the water of the urine in the blood, or its mere redundance. Often, also, where the quantity of urine excreted is very small, the skin, the bowels, and even the lungs, act *quasi* vicariously, and thus prevent or diminish any ill effects attributable to scantiness of evacuation of the watery portion of the urine. It is well known, also, that the skin will eliminate urea, in cholera, in such quantities that it not only can be detected, but the amount appreciated. (*Thudichum*, op. cit., *et alii*.)

We may now dismiss the watery element from our subject, and proceed at once to the consideration of the undue retention of the solid constituents of the urine in the blood.

UREA.—(Symbol: $\overset{+}{U}$.—Formula: $C_2H_4N_2O_2$.)[1]

This substance, "the principal product of the metamorphosis in the body of nitrogenized food," and always a constituent of healthy urine, is considered a blood-poison when retained in the circulation. Some observers

[1] Bird, Thudichum, *et al.*

Chemical Composition of Urea.

	THUDICHUM.	G. BIRD.
2C	20.000	$C_2N_2H_4O_2 = 60.$
4H	6.666	
2N	46.667	
2O	26.667	
	100.000	

believe its action to be direct, others that it is indirect—or exerted through the agency of a product of its decomposition. It forms the most considerable portion of the solids of the urinary secretion, and is purely excrementitious matter, the elimination of which by the kidneys is absolutely necessary to health and life. It is true that, in certain exceptional instances, large quantities of it have been ascertained to be present in the blood, for a long time, without compromising life, or even exciting those cerebral symptoms usually observed under such conditions; but it is to be presumed, either that the persons were, to a great extent, insusceptible of the action of urea, or else that the peculiar fermentation supposed to give rise to uræmic poisoning, by producing a noxious substance from the urea, did not take place. It is certainly very possible that certain persons may be less impressed by the presence of urea in the blood than others; but, we repeat, such cases must be entirely exceptional. With regard to the constancy of decomposition of urea when retained in the blood, and the consequent formation of another and a toxic substance, we have, as yet, too few facts to enable us to determine. If ever proved to be the rule, however, the intervention of certain unknown agencies might, in isolated instances, prevent its execution; and thus account for an apparent, or at least a temporary, immunity from morbid consequences.

After extirpation of the kidneys in animals, and in Bright's disease and some other affections, urea is found pervading many of the fluids of the body—as, the dropsical effusion, the blood, the perspiratory secretion, the vitreous and aqueous humours, and the liquor amnii.[1] Dr. Thudichum, who refers to its detection in the latter fluid by Wöhler, considers its presence there as exceptional; and is inclined, moreover, to throw doubt upon many of the reported instances of its occurrence in other fluids—the reports being in several instances merely assertions by the authors, and not ratified by proof, or else erroneously or partially quoted. Urea has also been declared to have appeared in the milk,[2] in the serum from blisters, and in the alvine evacuations of patients with diseased kidneys.[3] Dr. Rees states that he has "found most unequivocal evidence of its presence in peritoneal, pericardial, and pleural effusions, and also in the fluid of the arachnoid."[4]

With regard to the question, already alluded to, whether urea retained in the blood is directly or indirectly deleterious—that is, whether it acts *per se* as a poison, or becomes such by a process of decomposition, in which case the carbonate of ammonia is believed to be the injurious agent—there has, of late, been much discussion. The latter view has its zealous advocates, and their theory seems to be somewhat gaining ground. As we have already intimated, there are significant facts adverse to the conclusion that urea alone, as such, is a blood-poison. Dr. Bright remarked that urea may long exist in the blood, in renal disease, and yet no cerebral symptoms arise until the very last of life. He mentions one case which lasted from four to five years. Dr. Rees gives even stronger testimony. He found, in a patient who had no uræmic symptoms whatever, but who retained his cerebral functions to the last moment of his life, the blood more highly charged with urea than he had ever known it in Bright's disease. Dr. Johnson, of London, in his

[1] The presence of urea in the fluids of the body was first announced by Dr. Christison in the Edinburgh Medical and Surgical Journal, October, 1829.

[2] Rees, Diseases of the Kidney, London, 1850, p. 46. Albuminuria existed in the patient.

[3] Dr. Golding Bird—after the action of elaterium.—*Urinary Deposits.*

[4] Loc. cit.

justly celebrated work on diseases of the kidneys, affirms that no actual proof exists that urea is the poisonous agent, or, at least, that it is the only one. If admitted to have a poisonous influence, he holds that some peculiar, unknown condition of the blood must exist, to favour its toxic action. Frerichs is the author of the theory that carbonate of ammonia, resulting from a decomposition which the urea undergoes in the blood, is the poisonous agent. Its presence in the blood he indubitably ascertained, and by injecting it into the bloodvessels of dogs, he produced convulsions. Dr. Hammond, U. S. A., has made some interesting experiments, with the intention of testing this matter. He injected urea, vesical mucus, sulphate of soda, nitrate of potash, and carbonate of ammonia into the blood of dogs; in some instances removing the kidneys previous to injecting the substances. He did not detect ammonia in the breath of any of the animals operated on with *urea* by injection. He inclines to pronounce its presence in Frerichs's cases purely accidental. The animals from whom the kidneys were removed all died, after strong convulsions; and Dr. H. infers an analogy between animals deprived of the kidneys and patients affected with Bright's disease. Such analogy, it is true, may be predicated; but many attendant circumstances attaching to the cases of persons with diseased kidneys do not, of course, affect animals *without* kidneys, and consequently the analogy is not perfect, and hardly as safe to reason from as even the proverbially insecure foundation derived from analogical reasoning generally. Dr. Hammond does not find that urea or carbonate of ammonia, injected into the bloodvessels of sound animals, causes death; if they have suffered extirpation of the kidneys, such injection proves fatal. He does not discover from his experiments that urea, introduced directly into the circulation, becomes converted into carbonate of ammonia.[1] The experiments, at all events, go to prove the deleterious agency of urea, or of the product of its decomposition, when not promptly excreted from the blood, whether it be due, as in the case of the animals experimented upon, to loss of the kidneys, or, as in certain conditions in the human subject, to its retention in the blood by diseased, perverted, or obstructed action of those organs. While the question as to the exact material acting poisonously is still in abeyance, the *facts* relative to urea retained in the blood as productive of various "morbid effects," are indisputable, and as such we shall now proceed to examine them.[2]

GENERAL PHENOMENA REFERABLE TO THE PRESENCE OF UREA IN THE BLOOD.—From the fact that Bright's disease is the affection in which urea is most frequently retained in the blood, it will be all the more necessary not to refer any of its concomitant phases to the action of urea solely; although it is very plausible, and some of the best medical observers of the present day are beginning to teach, that many of the so-called *sequelæ* of Bright's disease may legitimately be referred to the presence of urea in the circulation. Thus, Dr. Watson remarks the extreme readiness of various organs of the body to become inflamed during an attack of Bright's disease. Especially is this found to be true, as all observers will testify, in reference to the serous and mucous membranes. Dr. Watson himself calls attention to this fact, and cites Drs. Bright, Christison, and Gregory to the same effect. He mentions, also, that M. Solon does not, in his volume on albuminuria, consider this tendency especially prominent in France.

[1] North American Medico-Chirurgical Review, March, 1858.
[2] See Appendix, note A.

Bronchial, pleural, pericardial, peritoneal, gastric, and intestinal inflammation are well-known and common *sequelæ* of Bright's disease, and occur, as to frequency, very nearly in the order above named. Now, it is very plausible to suppose that the abnormal condition of the blood, caused by the presence of urea, may be productive of many of these manifestations. Dr. Watson, while suggesting this, speaks particularly of disorder of the stomach and bowels, which so often follows or is concomitant of Bright's disease, and considers it may be explained by the action of "the poisonous material retained in the blood, and seeking a vent through supplementary channels of excretion."[1] He then refers, as corroborative proof, to the *post-mortem* appearances observed in these cases; "most commonly evident traces of disease are met with in various organs" besides the kidneys. This distinguished observer adds, that these manifestations "prevail with irregular frequency in different places. They are probably determined, in some measure, by local and peculiar agencies. Thus, vomiting and diarrhœa have been more familiar to the Edinburgh observers, than in London to Dr. Bright, or in Paris to M. Solon; while the headaches and coma so often witnessed by the British physicians have been comparatively uncommon in France."[2]

Although, in abnormal retention of urea in the blood, the vital tissues and fluids are all more or less affected, and, by a concomitant disturbance of the watery as well as of the solid portions, dropsical effusion may arise, yet the spinal cord and brain are the organs chiefly affected whenever urea becomes a blood-poison; and we have seen that the instances where it is not thus morbidly efficient, when retained in the blood, are exceptional. Generally speaking, when the amount of urea thus traversing the system is considerable, its effects are decided and rapidly disastrous. The affection is, properly, a cachectic condition; in other words, the system is, throughout, evidently depressed by a poison.[3] Were it not, moreover, that the disease of the cerebro-spinal system, consequent on uræmia, is usually so severe, persistent, and fatal, the blood would become very seriously altered in its constituent parts, and finally devitalized, by the retention of urea; and, in addition, the whole play of the vascular system would be disturbed. The large quantities of albumen often eliminated during the retention of unusual amounts of urea in the blood cannot be said to be referable to the presence of urea alone. Albuminuria and uræmia may coexist, but the union is not a necessary one; neither is directly causative of the other. This fact we find pertinently referred to by Prof. G. S. Bedford, in his short but instructive chapter on uræmia.[4]

If we were to investigate thoroughly the disturbed chemistry and proportions of the blood which might properly be imputed to retention therein of the solid constituents of the urine, the limits of an essay, such as the re-

[1] M. Claude Bernard (Leçons sur les Propriétés Physiologiques et les Alterations Pathologiques des Liquides de l'Organisme. Paris: J. B. Baillière, 1859) remarks that in renal disease, when the urine is suppressed, intestinal disorder supervenes. The bowels have taken up, so far as they can, the elimination of urea. The rule is, that there is an elective affinity manifested by certain glands in the elimination of certain products from the blood. When anything interferes with their action, others fulfil their office to the best of their ability.

[2] Lectures on the Principles and Practice of Physic, fourth edition, London, 1857, vol. ii. p. 682.

[3] Notwithstanding that many of the cerebro-spinal phenomena observed are those of irritation or excitation.

[4] Clinical Lectures on the Diseases of Women and Children.

quirements of the question now proposed seem to demand, would be soon attained, and, by the addition of the practical details, largely exceeded. We have interpreted the terms of the subject as indicating a desire for an exposition of the phenomena of disease believed to be legitimately referable to the abnormal retention of the solid urinary constituents; and whilst endeavouring to present these, most of the results of the disturbed proportions and composition of the blood will, in fact, be made evident. And, in concluding these general remarks, it is well to say that accomplished observers, some years since, have been inclined to ascribe many manifestations of disease of slight intensity, and previously obscure and very imperfectly understood, to the presence of abnormal quantities of urea in the blood.[1]

I. CEREBRO-SPINAL PHENOMENA ATTRIBUTABLE TO UREA RETAINED IN THE BLOOD.—(A.) A drowsy condition is often the first distinctly declared manifestation of toxæmia by the presence of urea or of the product of its decomposition in the blood. There are sometimes premonitory symptoms of less clear significance, but not always. Some writers, however, class them among the recognized *prodromata*.[2] This drowsy state ordinarily deepens into stupor and true coma, if it be impossible to relieve the blood of the offending element. The re-establishment of free diuresis, or a tendency to recovery from renal disease, may effect so desirable a result; but, unfortunately, the tide too frequently sets in the opposite direction. Coma may prove the final phase of the affection, and be simple or unaccompanied by disordered motility; or it may be combined with convulsions, and life may be terminated even more rapidly than if the complication had not existed.

(B.) Convulsions of an epileptic form may be the sole manifestation; there being no sopor, and consciousness being intact. The state is, in every respect, fully as unpromising as either of the two just indicated.

Among the first symptoms of uræmia may be mentioned—œdema in various parts of the body;[3] lowness of spirits, amounting at times to me-

[1] Urea is always present in healthy blood, but in very small proportion. It may sometimes even not be readily recognized. Morbid effects, consequently, depend upon its presence in large quantities, and upon its *accumulation*, in the blood. The abnormal increase, therefore, even if small, will exert an influence. For valuable chemical and physiological information on this point, see the works of Simon, Thudichum, and Carpenter.

[2] These will be specified hereafter; they are of a character less arrestive of attention than the others; we will only mention—confusion of ideas, failure of memory, unusual sluggishness, general *malaise*.

[3] This peculiar feature of the affection deserves especial notice. Its seats are chiefly the upper part of the body, the face, and the extremities—both upper and lower. The *labia majora* not infrequently exhibit it. Change of the patient's position often causes its disappearance, temporarily; and it frequently becomes less marked, or even vanishes, towards the end of pregnancy, even while the albumen of the urine and the structural disease of the kidneys is increasing. (Braun.) "The skin of the non-œdematous parts of the body appears very dry, and as white as chalk (chlorotic, hydræmic, leukæmic), and has a low temperature. Only those œdemata of pregnant women which exist contemporaneously with albumen, fibrin cylinders, and fatty degenerated scales of Bellini's epithelium in the urine, have a connection with uræmic eclampsia. The œdema of the lower extremities, ascites, and hydramnios, which are not complicated with albuminous urine containing fibrin cylinders, are not followed by uræmic eclampsia in pregnancy and labour. The affection of the kidneys with disease cannot certainly be inferred from the appearance of dropsy, as distinct causes may, at the same time, or one after the other, produce dropsies." (Braun, *Uræmic Eclampsia;* Duncan, p. 17.)

lancholia; restlessness; dizziness; headache; fretfulness; partial anæsthesia and delirium. Nausea, retching, vomiting, and rigors, are likewise noticed. There are often, also, impaired vision, amblyopia, muscæ volitantes, and amaurosis. These latter symptoms, as indeed all the others, are especially mentioned by Dr. Braun, of Vienna, in his late work on Midwifery; a chapter from which, devoted to Uræmic Eclampsia, has been ably translated by Dr. Duncan, of Edinburgh, and has furnished us with a great deal of new and valuable information. While Dr. Braun's views with regard to the subject of uræmic eclampsia will, doubtless, not be at present received as a whole by the profession, and are, indeed, questioned in many points by his translator, yet his extensive research, accurate observation, admirable description, and ingenious reasoning, render the chapter to which we refer at once entertaining, instructive, and full of practical suggestions. We acknowledge our great indebtedness to the author and his translator.

When stupor and coma finally supervene, a greater or less degree of apoplectic stertor accompanies the respiration. It is uniformly noticed that this stertor has a peculiarly *high tone*—a sort of shrillness, distinguishing it from ordinary apoplectic snoring. Reference was first made to this fact by Addison (*Guy's Hospital Reports*, 1839, No. VI.), and is repeated by Reynolds (*Diagnosis of Diseases of the Brain*, &c., London, 1855) and by Rees (*On Diseases of the Kidney*, 1850).[1]

The action of the poisonous agent in uræmia is believed by the best authorities (Tyler Smith, Braun, Reynolds, Churchill, *et al.*) to be first directed to the spinal marrow; and hence the sensitive impressions which make themselves morbidly apparent, as dizziness, headache, and subsequently convulsive movements. We are not to inquire into the *modus operandi* or etiology of uræmia, or of the other diseased conditions supervening in the human subject, upon retention of the elements of the urinary secretion in the blood; but—as we understand the question—to state the "morbid effects" only; consequently, we shall not occupy time and space by setting forth the received views and theories as to the direct or indirect modes of transmission of the deleterious influences, but will endeavour to state succinctly the disordered vital phenomena observed, and the pathological appearances, if any, which are noted *post mortem*.

[1] Dr. Reynolds, commenting upon this characteristic, writes thus: "The stertor exhibits a peculiarity first noticed by Dr. Addison. It is not of low, guttural tone, but of much higher pitch, and appears to be caused by the mouth rather than the throat, either by some position of the tongue against the roof of the mouth or teeth, or by some movement of the arches of the palate, not like that causing ordinary stertor, from which (although its mechanism is obscure) it presents the most obvious difference. (In several obscure cases—*i. e.*, obscure from the fact of the patient's not having come under notice until cerebral symptoms had appeared and consciousness was so far lost that no commemorative history could be obtained, and in which no œdema of the ankles was perceptible—this peculiarity of the respiratory stertor has at once awakened my suspicions; has led to an examination of the urine and the breath, and to the discovery in the former of albumen and fibrinous casts, and in the latter of an undue quantity of ammonia.") (*Op. cit.*, p. 110.) The latter fact is significant, in view of the doctrine of Frerichs as to the agent which proves poisonous in uræmia; and the experiments and observations of many others go far to confirm the opinion.

Dr. Reynolds adds: "The peculiar muscular condition causing this stertor, I am disposed to consider as the result of spasm rather than paralysis, and the spasmodic contraction may be either of sensori-motor, simply reflex, or tonic origin, forming only one of many phenomena which indicate excessive or perverted conditions of those groups of motor action. This hypothesis is, of course, as unimportant as the fact of the difference is valuable."

Uræmic symptoms may, of course, arise in both sexes from renal disease, or from mechanical obstruction to the excretion of the urine, as in hydronephrosis, retroversion of the uterus, urethral stricture, and closure of the ureters; which latter, if dependent on an unrelievable cause, must soon prove fatal. The occurrence of what has been termed "uræmic eclampsia" has been witnessed in non-pregnant females, and in males, and so cannot be considered as invariably belonging to the parturient state when urinæmia exists. The portion of Dr. Braun's work already cited, is devoted to the exposition of his belief that the convulsions observed during pregnancy are almost exclusively dependent on urinæmia.

After the appearance of the premonitory and of the earlier symptoms of uræmia, the progress of the mischief will, of course, be variable in different patients, and also according to the amount and *cause* of the retention of urea. Thus, it would seem natural that a large amount of urea being somewhat *suddenly* thrown into the circulation, and kept there by the continuance of the cause, should prove rapidly disastrous, and be accompanied with marked and violent phenomena. When gradually introduced, as in the slower advances of renal disease, or by the action of a progressive obstruction, the system may become somewhat accustomed to the presence of the deleterious agent. May not this be, in some degree, the explanation of the innocuousness of those very considerable amounts of urea the presence of which in the blood, and for a prolonged period, was ascertained by such accurate observers as Bright, Christison, Frerichs, and Rees; in conjunction, as we have previously intimated, with a possible greater power of resistance to the urea-poison in some constitutions than in others?[1] If this explanation be not in any degree admitted, the only alternative seems to be to accept the theory of Frerichs, that carbonate of ammonia is the toxic agent. In support of this view, we have the experiments upon animals, already referred to, in part, where extirpation of the kidneys was practised—as by Prevost and Dumas, Segalas, Tiedemann, Gmelin, Mitscherlich, Claude Bernard, Barreswil, Stannius, and Frerichs, all cited by Dr. Bedford to prove this point (*op. cit.*); and the test by injection tried by Bichat, Courten, Gaspard, Vauquelin, Segalas, Stannius, Frerichs; both methods without inducing convulsions. (*Idem.*) Dr. Bedford also mentions the significant fact that Vauquelin and Segalas proposed to give urea as a diuretic, so little did they consider it a poison! It is, under the present aspect of the subject, as well not to try the experiment.

Orfila—to come to direct experiments—caused fatal convulsions in an animal by the administration of carbonate of ammonia; and Bernard and Barreswil found carbonate of ammonia in the stomach and intestines of animals after extirpation of the kidneys.

Dr. Rees's idea that a peculiar "tenuity" of the blood may be requisite, in order to have full toxæmic action, when urea is retained, is certainly plausible; for we may at least suppose that the poisonous matter will be more readily and abundantly distributed through the circulating medium, and will consequently more thoroughly pervade and act upon the system.[2] And

[1] Frerichs states that the presence of the as yet unknown *ferment* in the blood is necessary, in order to the production of toxæmic symptoms by generation of the carbonate of ammonia. He thus explains the toleration of so much urea in certain cases.

[2] Dr. Todd ("Lumleian Lectures on Delirium and Coma," *Med. Gaz.*, 1850) also favours this idea. He believes the poisonous action of urea is facilitated by impoverished blood.

here we cannot refrain from adducing the exceedingly acute and ingenious remarks made upon this point by Prof. Simpson, of Edinburgh (*Obstetric Works*, vol. i. p. 371, American edition), in the article containing his statements in reference to puerperal convulsions, which latter, as we have already mentioned, recent observers have distinctly referred, in a large majority of cases, to toxæmia by the retention of urea, or of the product of its decomposition, in the circulation. In this particular connection, however, the patients were children—so that here we have remarkable instances of direct uræmic poisoning in connection with albuminuria—convulsions being the prominent symptom. The account of the first case we transcribe entire, together with a foot-note of much interest.

"A few weeks ago, I saw an instance in which convulsions in a child after birth were connected with the presence of albuminuria in its urine; or connected, as it should be, perhaps, more correctly stated, with that condition of the blood-poisoning or uræmia which is the result of albuminuria—whether that condition consists in a morbid accumulation of urea, or is produced, as Frerichs supposes, by the presence of carbonate of ammonia in the blood, produced by decomposition of the urea, or is, as is more probable, the effect of some other morbific agent in the circulating system, capable, like strychnia, of increasing the centric irritability or polarity of the spinal system to such an excessive degree that, under this super-excitability, comparatively slight eccentric causes of irritation in the stomach, intestines, uterus, bladder, &c. &c., readily induce convulsive attacks of a general form, like those of puerperal eclampsia." (*Loc. cit.*)

In the foot-note appended to the above passage, and in reference particularly to the theory of Frerichs, Professor Simpson makes the following important and interesting suggestions:—

"If the blood-poison, which in albuminuria produces convulsions and coma, be, as Frerichs believes, carbonate of ammonia, resulting from decomposition of urea, can we account for the power of chloroform in restraining and arresting, as it does, puerperal convulsions, upon the ground of its preventing this decomposition? The inhalation of chloroform produces, as various chemists have shown, a temporary diabetes; sugar appears in the urine, and hence, probably, also in the blood. The addition of a little sugar to urine *out* of the body, prevents, for a time, the common decomposition of its urea into carbonate of ammonia."

After mentioning the death of another child from convulsions supervening on the third day after birth—the mother having had puerperal convulsions and recovered—Dr. Simpson states that Dr. Weir, of Edinburgh, and himself found the urine of the child, like that of the mother, highly albuminous. He also says he is unaware of any reported observation of the coexistence of albuminuria and infantile convulsions; and then hints at the possibility that the albuminuria may be common as a pathological condition in certain forms of the convulsions of infants—as in trismus nascentium. Other infantile diseases, he thinks, may be powerfully influenced by albuminuria—as, for instance, sclerema, the "endurcissement ou l'œdeme du tissu cellulaire" of French writers. Dr. Simpson had only seen two cases of this in Edinburgh, but was led, at the time of observing them, to believe and "to suggest that the skin-bound disease itself, or at least some forms of sclerema, may be a variety or effect of Bright's disease in early infancy; the effusion into the cellular tissue, which constitutes the marked feature of the affection, being so far analogous to the anasarca occurring with albuminous nephritis."

In reference to the use of chloroform, and the explanation which Dr. Simpson attaches to its mode of action in overcoming puerperal or uræmic

convulsions, may we not ask whether the subduing power of the anæsthetic agent, acting as it does upon the cerebro-spinal system, directly, is not sufficient, of itself, to explain the control of the convulsive manifestations, without a resort to the exceedingly ingenious suggestion of Professor Simpson as to the chemical explanation of the result?

We may here remark that, in one case, Dr. Duncan, of Edinburgh, found that the inhalation of chloroform aggravated the stertor and lividity of countenance observed in a case of puerperal convulsions ("uræmic eclampsia" of Braun and others). We observe that the chloroform was administered in "small quantity"—perhaps Dr. Simpson might say the amount was not sufficiently large.

In a valuable note to a portion of the chapter of Dr. Braun's work which he has translated, Dr. Duncan has virtually enunciated the same opinions as Dr. Simpson's, previously cited—both in reference to increase of the nervous irritability acting on various organs, and to the analogy of action to be predicated from the experiments instituted by zealous students of these phenomena upon animals. We append his comprehensive and apposite remarks:—

"In uræmia, the most important point is the circulation of a morbid fluid in the nervous system, which probably does not act as a direct excitant of the convulsive motions, so much as it increases the irritability of the nerves, and the consequent liability to convulsions from exciting causes, which, under other circumstances, would produce no noticeable disturbance. Ingenious experiments have, as is well known, been performed on frogs, which seem to demonstrate an analogous condition to exist under poisoning by strychnia, at least when moderate quantities of the poison are administered." (*Loc. cit.*, pp. 59, 60.)

When the peculiar conditions to which the retention of urea in the blood is due, can be relieved and removed, we may witness rallying, and final recovery, even from very unpromising states. Persistence of the cause, however, by maintaining the presence and increasing the amount of the poison, soon induces the gravest accidents, and must terminate fatally, sooner or later, according to the violence of the attack, and the power of resistance manifested by the patient. It will serve at once the purpose of illustrating this fact, and of furnishing a synoptical view of the effects of urinæmia, to recapitulate and condense the phenomena observed under the established morbid conditions of the affection.

1. *External Appearance of the Patient.* (*Early Stage.*)—Aspect, that of general feebleness; and, if the depraved state of the blood follow scarlatina or Bright's disease, a more marked pallor, than when other causes are operative in retaining the urea in the blood—together with a puffiness about the cheeks and eyelids. Generally, sallowness, and anæmic hue, but sometimes blueness and congested appearance of the skin. More or less œdema of the extremities. Listless, confused, semi-idiotic manner.

(*Second Stage*).—Appearance that of a person apoplectically somnolent; degree, partial or complete; *modification*, by clonic contractions of the muscles.

(*Third Stage*).—Appearance that of one suffering from epileptic convulsions.

Either of the last two stages may be present singly, the other not occurring; or they may be combined and alternate.

2. *Disturbed Sensorial Manifestations. Early Stage.* ("Premonitory" of certain writers.)—Impaired vision; transient, partial and incomplete

amaurosis (Reynolds, *op. cit.*); muscæ volitantes; tinnitus aurium; temporary deafness.

(*Later Stages*).—Deficient, and sometimes entire loss of sensibility; complete amaurosis; permanent deafness—the latter less common. Sensation is seriously impaired, but not very frequently wholly lost. Distinct cognizance of impressions not taken; but usual appreciation of injury to the corporeal surface, felt. (Reynolds, *et al.*)

3. *Motorial Manifestations.* (*Early Stage.*)—More or less severe *clonic* contractions of the muscles; heavy and unwilling motions; slight stertor, "even when the patient is awake." (Reynolds.)

(*Later Stages*).—Voluntary movements mainly absent; sometimes to be provoked by excitation; continuance of clonic spasms; epileptic convulsions, more or less strongly marked. Dr. Reynolds (*op. cit.*) remarks that the rigidity of the muscles observed during this period varies greatly, being sometimes excessive, "and much increased by movement of the limb."

4. *Mental Condition.* (*Early Stage*).—Listlessness; fretfulness; uneasiness; confusion of ideas, impairment of memory, or its entire loss; partial or complete, but light, delirium—noticed often during sleep, or "when falling asleep." (Reynolds, Braun, *et al.*)

(*Later Stages*).—The profound insensibility of true coma, but at first capable of dispersion—the patient can, by persistent efforts, be aroused; soon, merging of this state into that of complete and irrecoverable carus. Frerichs notices the fact that the usually mild delirium which may, but does not uniformly, attend this state, is characterized by reiteration of the same word for a long time. A maniacal state may follow the coma, when that disappears.[1]

The species of coma first referred to—whilst the patient can yet be aroused—very much resembles that arising from opium, or other narcotic poisons, acting with full force. Dr. Reynolds, referring to this fact (*op. cit.*, p. 109), says he has noticed this sort of coma in the great majority of urinæmic cases he has observed. He writes:—

"The urinous element (whatever it may be) in the blood acts probably in a somewhat similar manner [*i. e.*, to that in which narcotics act]. There is not, however, in all cases of urinæmia, the notably contracted pupil that is observed in poisoning by opium. It is interesting to observe that the sensori-motor system appears to resemble, in its pathologic conditions, the spinal (or reflective) centre, rather than the cerebral (or intellectual). It is in a state of exalted rather than depressed activity, although both sensation and motion are severed from their purely cerebral relations (*i. e.*, from forming parts of perceptible and effective volition). There are several poisons which appear to act in a directly opposite manner upon cerebrum and spine (inducing at the same time coma and convulsions), but whether they contain different elements, whose action is thus separated, as Dr. Walshe once suggested, in a clinical lecture, the poison of urinæmia might be, I leave for future researches to decide."

5. *Special Functional and Organic Manifestations.*—In addition to the external appearances of the patient, as exhibiting deranged function of the skin, and perverted nutrition, the stomach and bowels may become excessively irritable. The vomited and other excreted matters, we are told by several observers, exhale ammonia when tested by hydrochloric acid; and the air expired from the lungs sometimes reacts similarly under the

[1] *See foot-note, page* 34.—"Some cases of puerperal mania, accompanied by albuminuria, and where no eclamptic attacks had occurred, are alluded to by Dr. Simpson."—*Duncan* (*note to Braun*, p. 136).

same agency (Frerichs, Johnson, Litzmann, Braun, *et al.*). The *pulse*, in the comatose state somewhat slow, rises, and is, at the same time, weak and irritable, in the convulsive periods.[1]

6. *State of the Urine.*—Confirmatory of the existence of obstruction to elimination and excretion. Depuratory processes at fault. The secretion is generally *acid* in reaction to tests, and *albuminous*—although cases of urinæmia occur in which albuminuria is not an element—casts of the *tubuli uriniferi*, and also blood-corpuscles and mucus-corpuscles are discovered by the microscope; and the urea is notably diminished in the specimens of urine passed. (Frerichs, Thudichum, Rees, Braun, Reynolds.)

A febrile condition, very similar to that of genuine typhus, is observed; and especially in connection with diminished excretion of the urine, or with its entire suppression. This is denominated by Frerichs, *febris urinosa;* the French writers designate it by the same term—"fievre urineuse." There is delirium, excessive prostration, and a urinous odour pervading the excretions; and death is then imminent. Death may, in certain cases where the blood has been exceedingly impoverished and contaminated, follow epileptiform convulsions which are due simply to the deteriorated and devitalized blood. These convulsions should be distinguished from those arising from other causes. Sometimes, even in such cases, rupture of cerebral vessels may cause apoplectic coma, by effusion of blood.[2]

Should life be prolonged, and in cases where recovery is possible, and occurs, there may remain permanent injury to the general health; or special organs may be particularly acted upon. Hemiplegia, hemerolopia, amaurosis, and impaired mental vigour may be mentioned. Œdema of the lungs and serous effusion into the cavities are noticed—the cerebral ventricles, even, being invaded by an urea-bearing serum. Life is too frequently destroyed, however, to enable observers to enumerate many cases and facts bearing upon this portion of our subject.[3]

II. POST-MORTEM APPEARANCES.—It is universally conceded that very few structural changes, of consequence, and often *none whatever*, are found on necroscopic examination—after death from the mere action of uræmic poisoning—in the cerebro-spinal system. The apoplectic effusions and lacerations of the cerebral substance are only indirectly, if at all, referable to uræmia. The condition of this system is that which chiefly concerns us at this time; for we are seeking strictly for the effects, both vital and *post-mortem*, legitimately due to the toxic agent derived from the presence of urea in the blood. A full description, therefore, of what is found after death in the *kidneys*, does not seem to us pertinent to the question; for the usual and well-known renal lesions of Bright's disease are not the product, but simply the frequent cause, of the uræmic condition. We shall, therefore, endeavour to particularize the appearances presented after death by those organs which during life most strongly manifest the effects of the toxic agent, and with which it is most intimately brought into con-

[1] It may be very much accelerated, and sometimes remains frequent throughout the affection.

[2] Drs. Watson, Todd, and George Johnson, have called attention pointedly to the fact of epileptiform convulsions springing from the circulation of impoverished blood in the cerebral vessels.

[3] The *mania* which sometimes follows the comatose state in uræmic eclampsia, is generally well recovered from; but should not be confounded with that symptomatic of puerperal pyæmia.—Braun; who refers to Helm, Litzmann, "and others."

tact and relation—mentioning more succinctly such other concomitant lesions as have been observed. And in noting the necroscopic manifestations in the cerebro-spinal system, we shall speak of such cases as have manifested the gravest indications of disturbance of the nervous centres during life, and especially decided coma and convulsions.

Brain.—Anæmic appearances, and a somewhat infiltrated condition, are noted; the consistence being most frequently diminished.[1] This state occurs irrespective of any abstraction of blood during life. The membranes of the brain are not commonly congested or "hyperæmic." Dr. Braun says, also, that inter-meningeal apoplexy is even more rarely observed than hyperæmia. He adds, that Helm and Kiwisch[2] very justly consider inter-meningeal apoplexy "as a secondary phenomenon produced by impeded circulation of the blood;" while it is looked upon by Litzmann as "a result of the uræmia."

Spinal Cord.—Examinations of the cord have been but infrequently made. (Romberg knew of *none* at the time of writing his *Treatise on Nervous Diseases*, the first edition of which was published in 1840 and the second in 1851.)

Braun states that Bluff, at one examination of the spinal cavity, "found much serum in it." Dr. Duncan reminds us, in this connection, that—as Dr. Christison first showed—the serum discovered in different regions of the body often has urea in it.

Dr. Todd, speaking of the condition of the cerebrum and spinal cord, after death following renal disease accompanied by coma and convulsions, says:—

"The organic disturbance of the brain which accompanies and causes the comatose tendency, is, as I have already remarked, much less than the pulmonary affection. There we find nothing which the most zealous morbid anatomist could call inflammation; and, except the patient may have died in convulsions, we do not even find congestion—that most fertile of causes with a

[1] Romberg (*Nervous Diseases of Man*, London, 1858, Syd. Soc. Ed.) when mentioning the results of cadaveric inspections in cases of *eclampsia parturientium*—which affection, it should be borne in mind, is now referred by such high authority to uræmic intoxication—says—rather adversely to what we find recorded by others—that in the cranial cavity we generally find considerable congestion, increased density of the cerebral tissue, plastic and sanguineous extravasations between the membranes, and in the ventricles, in the latter chiefly when apoplectic symptoms, a profound sopor, stertorous breathing, &c., have been associated with the convulsive affection." (*Loc. cit.*) The points in which this account chiefly differs from that given by Dr. Braun, are the frequency of congestion, and the increase of the density of the cerebral substance, which the latter observer distinctly denies, as indeed do others. Romberg, whose whole description of the epileptiform convulsions of the parturient female is admirable, clear, and vivid, refers on the above points to Hauck (*Einiges aus dem Gebiete der praktischen Geburtshülfe;* in Casper's *Wochenschrift der gesammten Heilkunde*, 1833, vol. i. p. 133; and Velpeau, *Die Convulsionen der Schwangerschaft während und nach der Entbindung. Uebersetzt von Bluff:* Köla, 1835, p. 86). Romberg, as well as other authors, refers to the paucity of necroscopic facts connected with eclampsia puerperalis; and especially with regard to the spinal cord.* This is, even at the present day, true; but the latest observations, coming as they do from reliable sources, must be allowed the greatest weight. With regard to the appearances in the *brain*, then, a certain difference of statement exists between Romberg and other authors we have consulted. As to the *spinal cavity* and *the cord*, there are no new facts within our cognizance.

[2] Helm, Th., Med. Jahrbücher. Wein, 1839, bd. xx. s. 202; Kiwisch, Beiträge z. Geburtsk. Würzburg, 1846.—Braun, *op. cit.*, chap. VI.

* Never had been examined when Romberg wrote; but has been since.

school of pathologists which is, I hope, fast disappearing. Indeed the brain is generally anæmic," &c. * * * (*Op. cit.*)

Dr. Simpson has reported (*Obstetric Works*, vol. i. p. 732) some cases of "puerperal convulsions connected with inflammation of the kidney," wherein effusion of blood and serum into the ventricles was discovered, with destruction of the right corpus striatum and outer portion of the optic thalamus, in one patient; together with encysted and degenerated kidney (morbus Brightii), and purulent-like matter, with adherent lymph, or false membrane, in another. In a third case, purulent-looking matter could be pressed out from the renal papillæ; no effused fibrin or coagulable lymph was discovered. The microscope did not decide the "whitish turbid fluid" to be pus—only epithelial cells, in large quantity, were found. The last patient had repeated convulsions, and died comatose, but no mention is made of any cerebro-spinal lesions being observed. The effusion of blood, and the laceration of cerebral substance in the first case related by Dr. Simpson, were truly apoplectic, and not referable to the intrinsic action of urea contained in the blood; although doubtless the amount present therein must have been considerable, since Bright's disease existed.

Dr. Watson (*op. supra cit.*, vol. i. p. 493), speaking of the appearances observed after death from apoplectic coma, after having referred to such a result from the action of retained urea, uses the following language:—

"On examining the brain, we may find a large quantity of extravasated *blood* spread over its surface, or lying within its broken substance; or a considerable effusion of *serous fluid* collected within its ventricles; or we may detect *no* deviation whatever from the healthy structure and natural appearance of the organ. The congestive pressure (if, indeed, it existed) has left no prints of its action."

The following observations, from the *Manual of Pathological Anatomy*, by Drs. Jones and Sieveking, are exceedingly apposite in this connection:—

"The researches of Bright, Frerichs, and others have demonstrated the close relation of the state of the blood to cerebral disease; and science has shown, what, previously, was purely hypothetical, that the most fatal conditions may be thus induced without any palpable changes being wrought in the cerebral tissues. It does not, however, follow, that because we see no changes, none have taken place. The poison that we know to be in the blood may elude our chemical tests, and yet cause death. Then, seeing how limited our knowledge of the nervous system is, it is not to be wondered at that, although the manifestation of altered function is so great as to force the belief in its altered constitution, it is not in our power to prove the latter to the perception; but, as Dr. Watson remarks, 'whatever may be the nature of the unknown, and, perhaps, fugitive physical conditions of the nervous centres, thus capable of disturbing, or abolishing their functions, it is useful to keep in our minds a distinct and clear conception of the fact, that there must be some such physical conditions.'"

Mere uræmia, therefore—presuming the occurrence of convulsions and coma—it would appear, leaves the brain anæmic in appearance, and possibly somewhat softened (more dense, according to Romberg; refer to page 35); the more decided and destructive appearances are owing to rupture of vessels, and consequent extravasation, with its consequences, and to dilatation of the ventricles with serum.

Lungs.—These organs are constantly found in an œdematous condition, and sometimes emphysematous. Dr. Braun, recording the fact that emphysema was long since observed by Böer, says that it is now considered

"as always the secondary result of the fits"—*i. e.*, uræmic convulsions. *Op. cit.*, p. 62.

Heart.—This organ is reported to be usually "empty and flaccid." (*Braun.*) We may thence infer feebleness of circulation, and impairment of its own tonicity, and of that of the bloodvessels, by reason of the impoverished state of the blood, their natural stimulant.

Kidneys.—Generally, and according to some authors, always, the kidneys exhibit more or less extensive and advanced signs of Bright's disease. In cases where the retention of urea in the blood has been caused by some other agency—such as obstruction, etc., there would naturally be traces of congestion, and perhaps of inflammation, although not uniformly; a nearly natural state might well enough exist.

As we previously intimated, it does not seem necessary to give, in this place, an elaborate account of the changes wrought by granular and fatty degeneration of the kidney, in connection with uræmia; and for reasons already stated. Moreover, these general appearances are well known, and abundantly set forth in many treatises. Those who would see, however, an admirable and somewhat condensed account of the changes of this nature effected in the renal tissue in urinæmic cases connected with pregnancy (*eclampsia puerperalis, seu gravidarum*), should consult the chapter of Dr. Braun's work, to which we have so frequently referred. A few extracts only will be made by us, and those chiefly to call attention to certain prominent points in the renal necroscopic phenomena.

Dr. Braun bases his descriptions on the three forms of Bright's disease proposed by Frerichs.

In the first stage, that of hyperæmia and commencing exudation, the surface of the kidney is smooth, the capsule is easily removed, the plexus of veins on the surface of the kidneys is dilated, and full of dark blood." (*Loc. cit.*, p. 62.)

"The pyramidal masses [renal papillæ] are likewise hyperæmic, and their injection is striped. The mucous membrane of the pelves and infundibula is swollen, and covered with vascular arborescence; and they contain a bloody fluid. Apart from hyperæmia, the finer structures of the kidneys do not appear to be essentially injured. Hemorrhagic effusions are very frequently observed, which sometimes take their rise from the glomeruli; sometimes from the vascular plexus of the tubuli uriniferi, sometimes from the veins on the surface of the cortical substance." (p. 63.) In the first stage, the epithelial lining of the uriniferous tubes is stated not to be essentially altered; the tubes themselves are often filled with exuded blood—"fibrin-cylinders."

In the second, or exudative stage, fatty degeneration commences and progresses, the kidneys becoming large, and heavier than they are normally. The capsule of the kidney can be easily separated. The pyramidal masses are dark red. The infundibula have a dirty-red mucous surface. The glomeruli (vascular knots, Malpighian corpuscles), which may be drawn out with a curved pin, are covered with a fine granular matter, and partly with solitary or grouped fatty corpuscles, which, by the addition of acetic acid, become transparent.[1] Between the glomerulus and the capsule lies a thick stratum of firm exudation, of granular structure, and having fat droplets, and sometimes crystals of cholesterine.

[1] Wedl mentions that acetic acid rendered the fat-globules more distinct.

The contents of the epithelial cells of the tubuli uriniferi next become degenerated—they are "decomposed in aggregations of granules."

Fig. 1.

Malpighian Corpuscle, from the kidney of a patient who died of *Eclampsia Parturientium*.—(From Wedl's Pathological Histology, p. 260.) "The surface is covered, partly with a fine granular substance, partly with solitary and aggregated fat-globules, which were not further changed by acetic acid or carbonate of soda. * *"—(*Loc. cit.*)

In the third stage, that of retrogradation, atrophy of the kidney is progressively induced. The pyramids of Malpighi and Ferrein are observed to be less atrophied than the cortical layer. At their bases, granulations occur between the straight tubuli, and press the latter apart.

The width of the renal pelves is increased, and their lining mucous membrane swollen and covered with a network of "varicose vessels, of an uniform grayish-blue colour."

"In those who die of uræmic eclampsia during pregnancy, atrophy of the kidneys is less frequently observed than the first two stages of Bright's disease." (*Op. cit.*, p. 69.)

III. URÆMIA IN CONNECTION WITH PREGNANCY AND PARTURITION.—If we receive the opinions of certain modern pathologists as true, the retention of urea in the blood of the pregnant female is productive, both to herself and to the fœtus, of the most disastrous results. The views of Dr. Braun, already referred to, have been adopted by a large number of able and practical men in our profession; and, since the publication of his volume—and mainly, in Great Britain and this country, through Dr. Duncan's translation of the chapter on uræmic eclampsia—have elicited great attention, and will doubtless prove an incitement to yet more extended and close observation.

Whilst many have joined Dr. Braun in this new theory, there were at the time his work appeared, and probably still are, several distinguished names arrayed against it. These are all mentioned by the author himself, who also states the objections made by them, and gives what he considers the refutations thereof. Dr. Braun's belief, as declared in his treatise, is simply this: *that the convulsions caused by uræmic intoxication in acute Bright's disease, and puerperal eclampsia, are identical.* This proposition, as he tells us, has been energetically defended by Frerichs, Litzmann, Wieger, Oppolzer, himself, and many others; but it has been assailed by Marchal, Siebert, Depaul, Legroux, L'Huillier, Stoltz, Seyfert, Levy, in very valuable articles, and also by Scanzoni. With much anxiety these observers have tried to prove that the Brightian degenerations of the kidneys, which, it cannot

be denied, are found in the bodies of those who have died of eclampsia, are consequences merely of the convulsions—only accidental, secondary phenomena of the hyperæmia caused by the eclampsia, and of hydræmia (*plethora serosa*).

Scanzoni, whose arguments against Dr. Braun's views are summed up and given by the latter author, refers the true *eclampsia parturientium* to an "irritability of the motor system of nerves which has been induced by pregnancy, and increased by the act of delivery."

The arguments *pro* and *con* have been actively carried on; and Scanzoni's conclusions were replied to by Wieger in June, 1854, and by Litzmann in 1855. In addition, the industry and zeal of Dr. Braun have enabled him to collect a truly imposing array of facts from *post-mortem* observations made by himself and other reliable practitioners, and in the majority of which abundant evidence of Brightian renal lesions existed. In some of the cases, where hyperæmia of the kidneys was found, microscopical examination was prevented by "accidental obstacles;" but the author does not consider this "any proof of the absence of Bright's renal exudation." Dr. Braun, while setting forth these investigations, says that Wedl[1] explains the non-discovery of fatty degeneration in the kidneys in several instances of death from eclampsia, "by the fact that in many cases a dissolution of the Malpighian bodies is effected by the fluid exudation, and hence in every diffuse inflammation of the kidneys an evident fat-metamorphosis of the contents of the Malpighian capsules does not ensue."[2]

Hasse, according to Dr. Braun, has never seen eclampsia puerperalis without Bright's disease. The necroscopic and microscopic observations of Wedl, Gustav Braun, Lumpe, Hecker, Devilliers, Regnauld, Simpson, Blot, Cahen, Wieger, Litzmann, Credè, Sabatier, and Hohl have contributed to establish the theory propounded in Dr. Braun's volume.

In reference to the *etiology* of "eclampsia parturientium" ("uræmic eclampsia" of Braun), Romberg[3] only hints at the agency which Braun has distinctly announced as, in his opinion, the chief efficient cause. Thus, while the latter ably demonstrates his views as to the retention of urea, and the accompanying albuminuria, the former refers to "retention of urine owing to pressure of the gravid uterus upon the bladder;" and afterwards adds: "Future investigations must determine whether albuminuria, which often supervenes during pregnancy, may, when fully developed during the last months, possess any etiological influence." The "investigations" of Braun and others certainly seem, if not already to have determined this point, likely to lead shortly to a satisfactory settlement of the whole question.

Supposing, then, in conformity with the new doctrines thus announced, that the convulsive attacks and intervening coma observed in puerperal patients are owing to toxæmia by retained urea, let us examine the collateral results of such a condition—first, in regard to the mother; and next, as respects the fœtus. Having already detailed the general effects produced upon the system by uræmic intoxication, we may properly direct our notice, at present, to the influence exerted upon the *puerperal state*, and upon the *life* of the mother and that of the fœtus.

Referring to our previous enumeration of the results, both vital and

[1] Grundzüge der pathol. Histologie, Wien, 1854, S. 306.

[2] Loc. cit., pp. 74, 75.

[3] A Manual of the Nervous Diseases of Man; Sydenham Society's edition, London, 1853, vol. ii. p. 189.

post-mortem, which are observed after genuine uræmic convulsions, we would only add thereto that the *uterus and its appendages* are generally found healthy after eclampsia puerperalis, or, at all events, do not necessarily deviate from their usual condition after labour has terminated; unless, of course, there has been some pre-existent or concomitant disease in, or alteration of, the organs. The infrequency of metritis and peritonitis is mentioned by some observers;[1] but Churchill[2] speaks of the great tendency to abdominal inflammation after the labour is over, and quotes Denman, who first mentioned this, and Collins and others, as confirming it. Braun also refers to the danger of puerperal affections coming on after eclampsia, especially if "an epidemic of zymotic diseases prevails."

The *spleen* is said to exhibit "the large dimensions it possesses in pregnancy and childbed."[3]

We now proceed to the consideration of the special influences exercised by uræmic intoxication during the puerperal state.

1. *Influence of Uræmic Eclampsia on the Duration of Pregnancy.*—Uræmic eclampsia is generally sudden in its accession, prompt in its results; and the testimony of those who have had the most experience in regard to it, is that it either causes death rapidly, or else that it is completely and with considerable celerity recovered from. Long consequent illness and sequelæ, *from the eclamptic state merely*, are not common. The true uræmic eclampsia occurs in the majority of cases in the latter part of the period of pregnancy. It may, also, only appear at the time of the labour itself; after the child is born, and before the after-birth has been delivered; or, finally, during child-bed. When supervening within the latter half of utero-gestation, premature labour is the result—as a rule.[4]

The latter two months of pregnancy are stated to be peculiarly obnoxious to the occurrence of convulsions. (Churchill, Romberg.) The *cause* of premature labour is either excitation of uterine contractions by the power of the abnormal action going on in the nervous centres,[5] or it may be, partially at least, ascribed to the presence of a dead fœtus, whose destruction is referable to the eclamptic condition. Ramsbotham, we observe, does not think the latter occurrence a cause. Churchill queries whether it may not be such; and the supposition is at least plausible. Dr. Copland (*Dict. of Med.*) speaks of cases where "the child has been unexpectedly born during the violence of the convulsions, as if expelled by them with unwonted celerity." Again, he states that the worst forms of the attack are often attended by a firm, spasmodic constriction of the cervix uteri, preventing the expulsion of the fœtus.

2. *Influence of Uræmic Eclampsia upon the Life of the Mother.*—Although puerperal convulsions—we here use the term, let it be remembered, as synonymous with uræmic eclampsia—are comparatively a rare affection, yet they make up for the element of infrequency of occurrence, by violence of manifestation and an alarming ratio of fatality. As the liability

[1] Romberg, Velpeau, *et al.*

[2] Theory and Practice of Midwifery.

[3] Braun, op. cit., p. 62.

[4] It is well known that Bright's disease, without any other influence, will cause premature labour; Braun says this is true in about 25 per cent. of cases. Add *convulsions*, and the danger is manifestly increased.

[5] Blundell (Principles and Practice of Midwifery, Am. edit., p. 418), referring to this point, says: "Sooner or later, * * if the fit continue, parturition commences of itself, without the interference of the accoucheur; and * * a sudden emersion of the fœtus occurs." He also refers to the occurrence of delivery during convulsions, unknown to the attendants.

to an attack is greater towards the last of the period of utero-gestation, so, generally speaking, is the danger to the life, both of mother and child, at that time. Not only is this true if we merely refer the eclamptic condition to the increased sources of irritability to which the nervous system has become liable, by reason of the pregnant state and its advanced stage; but also it is easy to see that a poisoned blood is all the more likely to act with increased morbid force—especially upon fœtal life. Again, the danger from convulsions diminishes according as they approximate to the term of delivery; and we are told, by a most competent authority, that the fits diminish in force in 31 *per cent.* of the cases, cease entirely in 37 *per cent.*, and only continue of the same intensity in 32 *per cent., after the uterus is evacuated.* (Braun.)

Dr. Braun refers to 15 deaths out of 45 cases, of which he has published accounts—being exactly *one-third.* He speaks of nine cases occurring in his practice within the last three years, all of which terminated in recovery. Thirty *per cent.* of the cases, it is estimated, prove fatal. Romberg gives a higher ratio of mortality. His statement is, that above one-half of the women attacked, died within twelve, twenty-four, or thirty-six hours. Churchill finds from his statistical investigations, that the proportion of fatal cases is above one-fourth. He intimates that there has been a tendency, of late, to diminution in the mortality-rate, which, at one time, he intimates, was very much larger. It has not been thought that much, if any difference as to fatality can be ascribed to early or late supervention of the affection; at all events, lateness of attack has not been allowed more weight, in this respect, by observers, when it is a first case. Some patients have several attacks in successive pregnancies, and finally die in one. A reiteration, therefore, of the accident, must be deemed of unfavourable augury. The concomitance of coma, with apoplectic stertor, and the approximation of the fits, so that they become, as is sometimes the case, almost continuous—and especially when these conditions obtain in plethoric and strongly constituted patients—are, almost without exception, fatal elements.

Recovery from the mere eclampsia may take place; and there may be some extensive dropsical effusion, some injury to the brain or spinal cord, or a permanent and increasing œdema of the lungs, disease of the heart, etc., which will compromise life at a later period; but, as a rule, if the patient escape the eclampsia, and its *immediate* results, recovery is usually not tardy, and, moreover, is complete. The occurrence of rupture of the uterus, in itself sufficiently grave at any time, we need scarcely say, very greatly aggravates the unfavourable prognosis attaching to eclampsia. Dr. Copland remarks (*Dict. of Practical Medicine*), that puerperal convulsions "should never be considered devoid of danger, more especially when they occur after delivery, or in consequence of great exhaustion of vital power, or of uterine hemorrhage. When they are slight, are unattended by stertorous breathing, or by paralytic or apoplectic symptoms, and when parturition is so far advanced as to readily admit of its completion by art, less danger may be feared."

The balance of prognostic opinion, it will be seen, is against recovery; and Blundell speaks of *post-partum* convulsions as being the most dangerous.

3. *Influence of Eclampsia on the Life of the Fœtus.*—We have already intimated the danger arising from eclampsia parturientium to the life of the fœtus. Relatively, it is even greater than that threatening the mother.

In referring to this point, those who have had the largest experience, use such expressions as the following: "In almost all cases the child is still-born, often putrid." (Churchill, *op. cit.*) "The life of the fœtus is endangered so long as it is nourished by the uræmic blood of the mother." (Braun, *op. cit.*) "The infant almost invariably dies when the disease occurs during the last months of pregnancy; it may be saved when the eclampsia supervenes during parturition." (Romberg, *op. cit.*, p. 189.) "The infant is generally, though by no means universally, born dead, when the woman has been the subject of convulsive seizures, especially if the attack has occurred early in the labour, and continued for any length of time." (F. H. Ramsbotham, *Principles of Obstetric Medicine and Surgery.*) The latter author pointedly and happily refers to *toxæmic action* on the blood of the fœtus as the most likely cause of its death *in utero;* and mentions a case from Spence, where the child being removed alive by Cæsarean section from a mother just dead from convulsions, died itself, in convulsive paroxysms, in less than an hour. The latter statement leaves us to presume a poisoned condition of the blood. M. Ménard states, that, in the majority of cases of death by convulsions, previous to delivery, the child has been found dead; the contraction of the features and extremities denoting that it had participated in the affection of the mother. Dr. Copland, who notes this remark (*Dict. of Med.*), says that it "wants confirmation."

We thus see that the likelihood of the life of the fœtus being maintained after eclampsia has been declared in the mother, is extremely small, as might, indeed, be expected; and if the child be born alive, there is great probability that it may either not survive long, or that it will be more or less morbidly affected by the accidents occurring during its intra-uterine existence.

Hereditary transmission of eclampsia, uræmia, or Bright's disease of the kidney, to a suckling, says Dr. Braun, "has not yet been demonstrated, and only Simpson has found albuminuria in a suckling born of an eclamptic mother." Dr. Duncan, in a foot-note to the paragraph from part of which the last remark is taken, says that "if the uræmia persists in a nursing woman, urea may be present in the milk, as has been shown by several observers, and may disturb the health of the suckling."

URIC ACID.—(Symbol: $\overline{U}$. Formula: $C_5HN_2O_3+HO$.)[1]

Synonymes.—Lithic acid; Urylic acid.—PROUT, BIRD, *et al.*

This substance is the next constituent of the urine, in the order of enumeration we follow; and the effects of its retention in the blood will therefore now engage our attention.

It is recognized as a constant ingredient of healthy urine, and holds a very intimate relation to urea. (Thudichum, *op. cit.*) "It forms less than

[1] Thudichum.

Chemical Composition of Uric Acid.

	THUDICHUM.	G. BIRD.
Carbon	35.714	$C_{10},N_4,H_4,O_6,C_2,H_4,N_2,O_2+2C_4,NO_2=168$
Hydrogen	1.191	
Nitrogen	33.333	
Oxygen	19.048	
Water	10.714	
	100.000	

$\frac{1}{2000}$ part of the urine in man; but its proportion varies much in different animals." (Dr. George Johnson, *op. cit.*, p. 50.) In the blood, it is always found in union with an alkaline base; and it is not soluble in that liquid. It appears in the blood in the form of urate of soda, or of urate of ammonia. (Liebig, Simon, Thudichum.) Dr. Thudichum states that the urates are always acid salts—*i. e.*, that there is excess of uric acid—and he has advanced some ingenious reasons for denying the accuracy of Dr. Golding Bird's theory and explanation of the secretion of uric acid in the form of urates. With this chemical discussion we have no concern; the indication of the pathological states induced by retention of the acid in the blood, in the form above specified, being now our object. Before approaching the subject in detail, however, it does not seem particularly out of place to allude to a practical remark by Dr. Thudichum (*op. cit.*, p. 100) upon the deposition of the urates from the urine. Researches and observations in reference to this point, cannot fail to be of importance in the treatment of such cases. Dr. Thudichum says:—

"If the presence of a deposit of urates be taken as an indication of the saturation of urine by these salts, and if the latter be assumed ordinarily to be of the usual amount, deposits of that kind become more valuable as signs of a diminished secretion of water by the kidneys than of any other symptom. As the appearance of a deposit of urates is always accompanied by morbid sensations and objective symptoms—in the healthy by thirst at least, if by nothing more—the conclusion is simple enough. *The individual whose urine has deposited the urates does not drink water enough, and must drink more, and must drink so much that the urine, at the ordinary temperature of the air, shall remain clear.* Of course, in some cases this will be neither possible or advisable; but in most cases of acute and febrile disease it should be a plan of treatment. I have certainly seen it attended by beneficial results in many cases; I have also observed the contrary—want of water in the system—to be a source of disease."

GOUT.—Whenever, from failure to eliminate the uric acid from the blood, that substance accumulates therein, the abnormal effects of its presence do not long delay their manifestation.[1] It is well known that the ingestion of large quantities of highly-azotized food, and a rich diet generally, together with the free use of malt liquors and of acid wines—Madeira amongst others—is productive of an abnormal increase of uric acid in the system; and, consequently, luxurious livers have long constituted the majority of sufferers from gout—a malady which abundantly declares its *fons et origo*, by the tendency to abundant deposition of the *urate of soda* in different parts of the body; its seat of election being the smaller joints—as those of the toes and fingers. In the latter, the deposit is often very plentiful. We have seen not long since—and the occurrence may not be very uncommon—a patient who could write his name with his "chalk-stone" knuckles; a woful example, truly, of diverted and arrested excretion![2]

[1] Bernard recognizes the accumulation of uric acid in the blood, either by arrest of the renal function, causing its elimination to cease, or by an exaggerated production of it, as in gout. (*Liquides de l'Organisme.*)

[2] An instance of this is related by Dr. Watson (*op. cit.*), and Dr. Todd has alluded to the condition. After all, cases of this extreme nature may not be so common as we have intimated.

Dr. Garrod (*On the Treatment of Gout and Rheumatic Gout*) remarks upon this point: "Comparatively few gouty patients become the subjects of visible chalk-stones, at least to any extent, or such as to induce deformity; at the same time, I am convinced that their occurrence in a slight degree is by no means so rare as has been generally assumed. Sir C. Scudamore stated, that in 500 cases of gout he only found them 45 times, or in less than 10 per cent. From my own expe-

As has been previously mentioned, Dr. Garrod, of London, first called attention, not only to the presence of uric acid in healthy human blood, but also pointed out the fact of its abnormal amount in connection with gout. He did not, however, then wish to be distinctly understood as declaring the acid the sole *materies morbi* in that disease, as may be seen by his remarks in a "Postscript" to his highly interesting paper, communicated to the Medico-Chirurgical Society, upon the subject. This important contribution to scientific medicine was read before the Society,[1] February 8, 1848, and the postscript just alluded to bears date July 26, 1848. (Vide *Medico-Chirurgical Transactions*, vol. xxxi.)

Daily observation tends to show that the relation of cause and effect may more and more safely be predicated in regard to the presence of an excess of uric acid in the blood and the phenomena of gout. Writers upon the subject, both near the time of Dr. Garrod's first researches, and later, have varied somewhat as to the completeness with which they have expressed themselves in respect to establishing uric acid as the active agency in gout. The majority of testimony seems to be affirmative. Dr. Watson (*Principles and Practice of Medicine*, 1848) seems to have then regarded the morbid agent as recognized. We find, indeed, in the edition of Dr. Carpenter's *Physiology* published in 1846, very positive language as to the conspicuousness of uric acid in gouty affections; he says: "When it [uric acid] is imperfectly eliminated, we are assured of its accumulation in the circulating fluid, by its deposition, in combination with soda, in the neighbourhood of the joints—forming gouty concretions, or chalk-stones." He thus appeared to recognize the cause of the diseased condition as lithic acid. There are those, however, who, even at the present day, speak with less distinctness as to an excess of uric acid being the sole and sufficient *materies morbi*. Thus, Dr. Barclay (*A Manual of Medical Diagnosis*, London, 1857) writes: "The researches of recent times have gradually led to the discovery of an important element in gout—the presence of an excess of uric acid in the blood. This knowledge holds out a prospect of our arriving ultimately at more accurate diagnosis; at present, it is only in the hands of a few that such a chemical test can be relied on." The opinion is a very guarded one—decidedly non-committal; we think more influence than it implies may safely be allowed to the "element" in question.

It will, at least, not be disputed that gout is a blood-disease. Amongst many other observers who might be cited on this point, we select Dr. George Johnson, as furnishing comprehensive testimony. Referring to gout as a cause of renal disease, he says: "It would be useless to occupy the time of my readers by lengthened arguments to prove that gout is a blood-disease, since all the phenomena of the disease clearly indicate such an origin, and can be explained on no other supposition." (*Op. cit.*, p. 78.) He then alludes to the intimate connection between gout and the uric acid diathesis. Thus, then, when such a diathesis prevails, or when, by some obstructing agency, the elements of the urinary secretion are retained and accumulate

rience, I consider these numbers far below the real proportion, being confident that their existence is frequently overlooked, as they are sometimes deposited in parts of the body scarcely to be expected." Dr. Garrod thinks that gouty concretions are more frequent upon the cartilages of the ear than anywhere else; contrary to what has usually been recorded. He refers to the *Medico-Chirurgical Transactions*, vol. xxxvii., 1845, pp. 74, 75.

[1] By Dr. C. J. B. Williams, upon some of whose patients Dr. Garrod's experiments were made.

in the blood, the gouty accidents, amongst others, prevail. If uric acid be prominent, the corresponding series of symptoms seems as sure to occur, as does that following the retention of urea when *that* substance is retained, in excess, in the circulation.[1] If the uric acid, therefore, is received as the true *materies morbi* in gout, we have, at once, the following easily-deduced sequence of morbid effects:—

First, deficient depuratory action; next, accumulation of uric acid in the blood; *dyscrasia*, chemical and physiological; deposition of urate of soda in various parts; the objective phenomena of gout; the *sequelæ* of gout—amongst others, as Dr. Johnson points out, renal disease. Under the light of modern pathology, we do not think the above any too weighty a burden to lay to the charge of retention and accumulation of uric acid in the blood.

There may remain a certain quantity of uric acid in the urine, at the same time that the analysis of the blood shows an abnormal amount therein. This would indicate a large supply from some source—either from waste of the tissues, or from the excessive ingestion of highly-azotized food, to which latter cause we some time since alluded. Dr. Copland notices, in a comprehensive and satisfactory manner, the "Pathological Relations of Uric Acid and Urate of Ammonia" (*Dict. of Pract. Med.*); and M. Becquerel (*Sémiotique des Urines*) agrees with him in his views. Copland, referring to this, considers the chemical explanations offered by Liebig, in connection with certain of these pathological points, "opposed to clinical observation."[2]

Dyspepsia, with mal-assimilation of food and consequent deficient nutrition, or arrested cutaneous function and habitual costiveness, no less than obstructed excretion of the urine, may engender an excess of uric acid, and the latter may be retained in the blood. (Copland, *loc. cit.;* Chambers, *Digestion and its Derangements; et al.*) It is likewise true, conversely, that in depraved, deteriorated, and anæmiated states of the circulation, uric acid is diminished in the urine. If this ill-adjusted balance implies the throwing of an unusual quantity of the substance into the blood, the latter circumstance may have no small amount of influence, if not in causing, at least in continuing, the disorder of the blood itself and of the constitution generally. Gout occurring under such circumstances—as it is not very infrequently known to do—would be appropriately termed *atonic gout*, or what Dr. Todd (*Clinical Lectures*) terms "*asthenic gout*," in contradistinction to his "sthenic gout," where the affection occurs in robust, well-constituted, and richly-nourished individuals.[3]

In reference to the dyspeptic condition of gouty patients, often so exceedingly troublesome, Dr. Chambers (*op. cit.*, American edition, 1856, pp. 294–95) refers rather scornfully to the influence of uric acid as a noxious element. He is remarking upon the tendency of the food to become acid after ingestion, and to lie unchymified—not "passing onwards." This

[1] The blood contains, as Dr. Garrod remarks, "mere traces" of uric acid *in health*. This fact, however, in no degree invalidates the agency of the acid as *a causative element in gout*.

[2] Dr. Copland does not believe that the presence of urate of soda in the blood of gouty patients precludes "the elaboration of a portion of the uric acid and its compounds, or the modification and metamorphosis of one or more of their elements by the kidneys." (*Dict.*)

[3] These two forms of gout, we conclude, are those termed by Dr. Druitt "*high*" and "*low*" gout. (*The Surgeon's Vade Mecum*, 1859.)

state of things, so common in hereditarily gouty persons, Dr. Chambers is inclined to explain by considering its pathology "to be a slight flux of mucus, deficient gastric secretion, and yet a vigorous, sometimes even excessive appetite. Hence, they have not that check of failing desire for food which makes the meals of other invalids moderate, and eat more than their imperfect gastric juice can digest. This is a simpler, and, therefore, more probable explanation than the usual chemical talk about uric acid, &c., which might be substituted for it." It seems there is some fault in the working of the hidden *chemistry* of the body, however; and, although Dr. Chambers's explanation is doubtless very correct in reference to the influences and agencies of which he speaks, yet the overt action and manifestations of excess of uric acid, in the visible form of urate of soda concretions, sufficiently show the importance of the part it plays in the *tout ensemble* of gouty affections.

The original conclusions of Dr. Garrod, given in the admirable paper to which we have referred, were these:—

"1st. The blood in gout contains *uric acid* in the form of urate of soda, which salt can be obtained from it in a crystalline state.

"2d. The uric acid is diminished in the urine immediately before the gouty paroxysm.

"3d. In patients subject to chronic gout with tophaceous deposits, the uric acid is always present in the blood and deficient in the urine, both absolutely and relatively to the other organic matters, and the chalk-like deposits appear to depend on an action in and around the joints, &c., vicarious of the 'uric-acid-excreting' function of the kidneys.

"4th. The blood in gout sometimes yields a small portion of urea (no albumen being present in the urine)."

These conclusions were all duly sustained by analyses and experiments upon patients in University College Hospital; and the record of these demonstrations is at once satisfactory and highly interesting.

In respect to the supervention of gout in low and debilitated states of the system, to which allusion has previously been made, Dr. Garrod very clearly explains "two facts" then considered opposed to referring the pathology of gout to the humoral doctrines. We may say, *en passant*, that the humoral pathology seems the only reasonable mode of explaining the affection, and that, as may be distinctly perceived, it is coming more into favour of late, than it has been for a long time, in respect to many diseased conditions.

Dr. Garrod's remarks on the above point are:—

"Any undue *formation* of this compound (urate of soda) would favour the occurrence of the disease; and hence the connection between gout and uric-acid gravel and calculi; and hence the influence of high living, wine, porter, want of exercise, &c., in inducing it." Then, speaking of the "two facts"—viz., *hereditariness* and the supervention of gout in *low states* of the system—he says: "We can understand that the peculiarity of the kidney, with reference to the excretion of uric acid, may be transmitted; and likewise that, when the function in question is permanently injured [viz., the 'uric-acid-excreting' function], it will not require an excessive formation of the acid to cause its accumulation in the blood." (*Loc. cit.*, pp. 93, 94.)

Dr. C. J. B. Williams (*Principles of Medicine*), referring to the fact that gout had been generally admitted, by inference, to depend on the existence of an excess of uric acid in the system, chronicles Dr. Garrod's experiments and analyses, which, as we have stated, were first made on patients under Dr. W.'s care, in hospital. He says:—

"Gout, and the commonest kind of urinary gravel are now generally considered to depend on the production in the system of an excess of lithic acid." (*Loc. cit.*) The case from experiments upon which Dr. Garrod was first enabled to draw his reliable conclusions, "was one of chronic gout; and further illustrated the pathology of the disease, by a total absence of lithic acid in the urine, until during the exhibition of colchicum, when its characteristic crystals appeared under the microscope."

Sufficient testimony, it would appear, has been adduced, to render the position tenable which ascribes the gouty paroxysm to an excess of lithic acid, circulating in the blood, and finally deposited in various parts of the body.[1] It does not seem to devolve upon us to describe the phenomena of a fit of the gout—expected as we are, merely to signalize the "effects" of retention of the various elements of the urinary secretion in the blood, we should strictly confine ourselves to such specification, and to the adduction of the best evidence afforded in its support.

In the first place, then, we may refer the phenomena of gout, more or less completely, to that disorder of the eliminating function of the kidneys, which permits the latter organs to refuse the excretion of uric acid. The latter substance is then necessarily thrown into the circulation, and its tendency—under the circumstances—is to accumulation in the blood. As it accumulates, it is converted, most commonly, into urate of soda, and the deposition of the latter substance upon and into various tissues of the body is a *quasi* vicarious discharge of the uric acid, not excreted by its legitimate channels, the kidneys. This condition is accompanied by the objective phenomena of gout, viz., pain, of an exquisitely acute and torturing character; feverishness, dyspeptic symptoms, and general *malaise*;[2] at the close of the paroxysm, tense, shining, and often excessive swelling of the affected part; finally, entire remission of the symptoms, and better health than before the attack—owing, of course, to the elimination of the *materies morbi*. The *subsequent course* of things will depend very materially upon the habits of the patient, and upon the power he has of restraining his appetites; the fact of hereditary predisposition, or the contrary; and whether the management of the initial attack is judicious, or otherwise. By renewed attacks—chronic gout—the system of course becomes more shattered, less capable of resistance, and less amenable, too often, to remedies. Locally, a variable, often an excessive, amount of injury supervenes. It is at these periods that the lithate of soda—the morbific material—is deposited in various places—principally about the smaller joints. Nature, in her efforts to eliminate this material by other channels than those which are the legitimate ones, does the best she can, but often terribly cripples the subjects of this trying affection (see Fig. 2). Connected with this chronic form of the complaint, we are apt to notice the most troublesome combinations of dyspeptic ailments; and in these the condition of the patient becomes the most unpromising possible, both from the local and from the systemic difficulty. As Dr. Williams states, the uric acid, in such cases, "seems to be engendered in great abundance, and although thrown off in large quantities in the urine for an indefinite period, yet never leaves the body free. Such cases are commonly either hereditary, or those which have been ren-

[1] See Appendix, Note B.

[2] "An impure state of the blood, arising principally from the presence of urate of soda, is the probable cause of the disturbances which not unfrequently precede the seizure, and of many of the anomalous symptoms to which gouty subjects are liable." (A. B. Garrod, *op. cit.*, p. 341.)

dered inveterate by intemperate habits, or neglect of proper treatment." (*Op. cit.*)

Fig. 2.

Elbow and Hand deformed by Gouty Enlargements.—From Dr. Garrod's work (Nature and Treatment of Gout.)

Sequelæ of Gout.—Considering gout as one of the affections ascribable to the retention of uric acid in the blood, let us inquire what are the principal subsequent results. These may, of course, be properly referred to the same cause, as "morbid effects."

We have seen that symptoms of general febrile disturbance, which are of course accompanied by nervous apprehension, fretfulness, heat of skin, more or less sleeplessness, and scanty urine—and which at last is loaded with the lithates, whenever the paroxysm of gout comes on, and at its decline, especially—with hypochondriacism, cramps, flatulence, diarrhœa, but more commonly costiveness (Watson), and more or less general and indescribable *malaise*, both precede and accompany gouty attacks. The dyspeptic symptoms may long remain; although, with care, they may be made sooner or later to disappear.

One of the most serious sequelæ of gout may be considered that wherein the kidneys are affected. Perhaps we may best describe the state we wish to indicate, by terming it the result of a sort of *recoil* upon the kidneys, of the antecedent morbid action in the economy. This is of course entirely the opposite condition of that obtaining when the kidneys fail to eliminate the uric acid, *i. e.*, when their "uric-acid-excreting function" is suspended. In the latter condition of things, we have the blood-disease, the results of which we have sketched above; but when the uric acid begins again to be excreted by its natural passages, the kidneys are very likely to be more or less irritated in the process. There may then be violent pain (*nephralgia*), and nephritis or true inflammation of the substance of the organs may occur. Fully as unfortunate is that state when there is such an amount of uric acid as to be thrown down from the fluid part of the urine—the latter not being sufficient to hold it in solution, and thus carry it out of the body—when it is exceedingly apt to become concreted in various parts of the urinary passages—thus producing obstructions more or less seriously endangering health, and vitiating, in different degrees, the integrity and usefulness of the organs involved. Permanent irritation may thus be maintained; or serious and fatal inflammation be set up; and concretions may be found in

the urinary bladder, which will call for surgical interference for their removal.

Dr. Williams (*op. cit.*), referring to the renal irritation frequently caused in these cases (p. 130, American edition, foot-note), says: "I have in several instances found in the cortical and tubular structure of the kidney, clustered crystals of lithic acid, which, under the microscope, exhibited such sharp angles and dagger-shaped projections, as would afford an easy explanation of the pain, inflammation, and hemorrhage, often attendant on an attack of renal gravel, even when none is obvious in the urine." The same writer reminds us (p. 187), of "the proximity in composition between lithic acid and urea," and that it is probable, according to Liebig, that the former may be converted into the latter. He likewise calls attention to the fact that both gout and rheumatism may give rise to fluxes and catarrhal affections, as oliguria does. Rheumatism and Bright's disease are also, often, nearly related. In respect to this fact, Dr. Garrod announced the following opinions and conclusions, in his paper already quoted (*Med.-Chir. Transactions*, vol. xxxi.), "Blood from patients suffering from Bright's disease and albuminuria after scarlatina, was then examined; the results of these analyses appear to show that—

"1st. Uric acid is always present in the blood in albuminuria. The quantity, however, greatly varies: when the functions of the kidneys are much impaired, it exists in quantities almost as great as in gout; in other cases its amount is small, but it usually exceeds that found in ordinary blood.

"2d. Urea always exists in large quantities in this blood (a fact which has been long since proved), and no relation is found between the amounts of urea and uric acid.

"3d. The kidneys are always deficient in their power of throwing off urea; but with regard to the uric acid, their excreting function may be impaired or not." (*Loc. cit.*)

We thus see what serious disturbances may arise in the system by a perverted condition of secretion, arrest of excretion, and attempts at vicarious elimination of a product which must, in order to the preservation of health, be discharged from the body. The diseases thus produced come clearly under the head of *disordered vital chemistry*. Thus, when the above vital functions are weakened, or totally disabled, there must be not only general disturbance, but, after a time, some special manifestation of disease, and the results of vitiated secretion, decomposition, and over-worked and irritated organs.

The *contracted kidney*—called, by the late Dr. Todd, "the gouty kidney"[1]—and which, as the term he has given it implies, he considered due to the effects, at once irritant, inflammatory, and destructive, arising from gout, we may mention as a result primarily dependent on disorder of the "uric-acid-excreting" function. Dr. Todd has given satisfactory proof (*Clinical Lectures on Certain Diseases of the Urinary Organs*) from cases of patients, of the existence of this form of renal disease. He mentions, also, that many might be inclined to refer it to advanced Bright's disease; but he has signalized it in those who he believes never had that affection. The kidney is shrunken, "and its structure condensed—a condition which, while it may also occur in other states of the system, is

[1] A term objected to by Dr. Barclay (*op. sup. cit.*); who, however, very distinctly recognizes the close connection between gout and renal disease. Valleix, writing in 1853, would not admit that special form of nephritis which is referred to the gouty diathesis.

peculiarly apt to be developed in the inveterate gouty diathesis." (*Loc. cit.*, p. 313.) In one case, necroscopy disclosed hypertrophy of the heart with dilatation—partly due to incrustations on the mitral valve—hardened, condensed, and somewhat contracted liver—the Glissonian capsule being hardened and thickened. The morbid alteration in the latter organ is explained partly by the intemperate habits to which the patient was addicted, "but partly likewise by the share which the liver had in the elimination of the morbid poison of gout." This latter result, which is not infrequent, it is important to notice, as one of the secondary effects of the diverted and retained urinary element. The kidneys in this patient were very much contracted, being hardly one-third of their natural size; they were granular and shrivelled upon their surfaces; the investing capsule was apparently denser and whiter than usual, and was easily detached from the glandular surface. Diminution of the cortical renal substance was the source of the decrease in the size of the organ; two-thirds being estimated to have disappeared. The granulations were noticed likewise upon the cut surface. Dilated and scantily-lined *tubuli uriniferi* were observed on microscopic inspection; from some of them all the epithelial lining had vanished; others were collapsed, folded, and crumpled up; and looked like "fascicles of fibrous tissue." A few fatty epithelial cells were detected in certain *tubuli;* and others of the latter were seemingly healthy, especially those in the pyramids of Malpighi. Dr. George Johnson[1] has described, very minutely, this condition of the kidney, and particularly notes the changes which supervene in the vascular system of the gland. In another instance, Dr. Todd has recorded the discovery, in an inflamed "gouty kidney," of "*opaque streaks of deposit of lithate of soda*" in some of the renal cones. These streaks took the direction of the tubes, and certain of the latter were probably occupied by them.[2] We may well exclaim with Dr. Todd, as we reflect upon these vital manifestations and post-mortem revelations—

"How strikingly do these consequences of the long continuance of the malady comport with the humoral view of the pathology of this disease! Not only are those parts which the morbid matter of gout is most prone to affect, materially damaged, but likewise the emunctories through which the poison would make its escape out of the system—the liver and kidneys: these organs have become poisoned by the morbid matters which have escaped, or tried to make their escape from the system through them; and, therefore, it is natural to expect a considerable change in their nutrition."

It is notorious, however, that in most, if not all, of the *metastases* of gout, there are no traces of morbid action upon the organs affected by the repercussion of the disease. The reason of this doubtless is, that when a fatal result occurs in this manner, the morbid action lasts too short a time, notwithstanding its violence, to leave structural traces. Probably a true *spasmodic* action often destroys the patient, in the thoracic and abdominal varieties of retrocedent gout. It is, however, very likely that in many cases of sudden death from these causes, the vital organs have been previously weakened by disease of some sort; and in many cases chronic gout has inflicted a certain amount of injury, for, as we have already seen, the continued and recurring malady *does* leave its indications, very decidedly, behind it.

[1] On Diseases of the Kidney.

[2] This appearance is also noted by Dr. Garrod, who saw "streaks of white matter at the apex of each pyramid and running up in the direction of the tubuli. Kidneys pale and contracted; cortical portion shrivelled." (p. 199.)

After gout in the *stomach*, Dr. Todd signalizes a "dilated and flaccid state of the organ" as existing, and this is the more marked in proportion as the attacks have been frequent.[1]

In addition to the abundant and often astonishingly copious deposits of the urate of soda into and around the joints, that salt has been found to cover and even to penetrate into the texture of the cartilage investing the affected joints (Watson), and to insinuate itself into the substance of tendons and ligaments (Dr. Wm. Budd). A curious locality is at other times chosen by it—viz., under the skin covering the cartilages of the ears. It has been remarked also over the cartilages of the alæ nasi (Todd).[2] Pus sometimes forms around the variously located depots of urate of soda, and occasionally the discharge of this liquid is quite abundant. Generally speaking, also, the joints of the hands and arms exhibit more plentiful deposits of the urate than those of the lower extremities, except in some forms of acute sthenic gout, when the reverse may be observed. The interference with the motion of the various joints thus affected is so evident and so familiar to practitioners, and indeed so well known to nearly every one, that we hardly need do more than allude to it as a "morbid effect" of the disease. The fact, moreover, has already been made sufficiently prominent.

[1] Op. cit.

[2] Dr. Garrod (op. sup. cit.) states that "sometimes small nodules of urate of soda are found upon the eyelids, especially the lower, now and then in the integuments of the face." He also refers to the great care which is necessary in ascertaining the precise nature of the "white-looking deposits" often occurring about the eyelids and face. Dr. H. Barker saw them on the nose. Dr. Garrod has "observed a true gouty deposit as large as a split pea, apparently attached to the fibrous structure of the corpus cavernosum penis." (p. 86.)

In relation to gouty affections of the eye and ear, Dr. Garrod further remarks: "A form of ophthalmia connected with gout has long been noticed." He adds that it is liable to be confounded with rheumatism, when that is directed to the ocular region. Cases of ophthalmia evidently connected with the gouty diathesis have been observed by Dr. G.; these were instances of conjunctivitis and sclerotitis. "Gouty iritis is also said to occur." The nodules found upon the cartilages of the ear have been mentioned. "Deposits are not unfrequently found upon the drum of the ear, and have been especially pointed out by Mr. Toynbee, but I have failed to discover a trace of uric acid in several examinations of them. Whether they are ever connected with gouty inflammation, is at present a matter of uncertainty; they should especially be sought for in gouty subjects in whom the joints are much affected with chalk-stones, for if not found in such cases, it is not probable that they would occur in others." (pp. 515, 516, op. cit.)

Accomplished aurists have pointedly alluded to the connection between gouty and rheumatic affections and deafness. Mr. William Harvey (The Ear in Health and Disease, London, 1856) specially considers this subject; and we have heard him insist upon the frequency of the connection, whilst observing his aural practice.

In his elaborate work on The Diseases of the Ear, just published (London, 1860), Mr. Toynbee refers to the subject, and furnishes interesting illustrations of the reality of the connection, as observed by himself. He says: "The poison of *gout* may also give rise to deafness and other peculiar symptoms in the head." (See the work cited, page 362, English edition.)

(*To be concluded in next No.*)

REVIEWS.

ART. XIII.—*Statistical Report of the Sickness and Mortality in the Army of the United States, compiled from the Records of the Surgeon-General's Office; embracing a period of five years, from January, 1855, to January, 1860.* Prepared under the direction of Brevet Brigadier General THOMAS LAWSON, Surgeon-General United States Army. By RICHARD H. COOLIDGE, M. D., Assistant Surgeon U. S. A. 4to. pp. 515, with a map.

THE issue of this volume, following those of 1840 and 1856 (the former comprising the record of twenty, and the latter of sixteen years), completes the sanitary history of the United States army for forty-one years—from 1819 to 1860. An identity of arrangement between the report published in 1856 and the present one, has enabled the careful and industrious compiler, Dr. Coolidge, to consolidate all the aggregate results for the period of twenty-one years. The meteorological register for the term of five years, ending with 1859, is also appended; giving, with those contained in the previous reports, a continuous series of meteorological observations, taken in all parts of the country, from the organization of the Medical Department of the army to the present time.

The abundance of topographical details presented in the volume of 1856[1] has rendered unnecessary the accumulation of much matter of the same kind in this work. But, in several of the local reports, especially those from Florida, New Mexico, California, Oregon and Washington Territories, and Utah, very interesting information is afforded, in regard to the natural history and population of distant regions.

Among the reports from the Northern Division, an account is furnished by Assistant Surgeon La Fayette Guild, of the occurrence of yellow fever in July, August, and Sept. 1856, at Fort Columbus, on Governor's Island, in New York harbour. No meteorological peculiarities of the season were noted; and, although the buildings in which most of the cases occurred were somewhat deficient in cleanliness, yet the fact that *one side only* of those buildings (that towards the quarantine) was thus infected, conspired with other circumstances to show that the disease was the result of the atmospheric transmission of its cause from the numerous vessels congregated at quarantine. The same opinion has been elsewhere asserted by Assistant Surgeon J. H. Bailey, in regard to the contemporaneous epidemic at Fort Hamilton. Surgeon C. McDougall explains similarly the occurrence of yellow fever at Fort McHenry, Maryland, in September, 1858; but Surgeon B. M. Byrne prefers the theory of contagion, to account for its existence at Fort Moultrie, S. C., the same season.

In the fourth quarter of 1858, an epidemic catarrh prevailed at West Point, thus described by Surgeon S. P. Moore: "The stress of the disease fell upon the fauces, tonsils, and adjacent parts. An examination of

[1] Reviewed in this Journal, July, 1857.

the throat showed an extensive erysipelatous condition of the mucous membrane." The same affection, proving fatal in several cases among the inhabitants of the neighbouring country, was designated by them "the black tongue," or "malignant sorethroat." At Fort Ridgely, Minnesota, in November, 1857, a similar epidemic was described by Assistant Surgeon N. S. Crowell:—

"In the majority of cases, the tonsils were more or less inflamed, and sometimes enormously swollen The inflammation in many cases was intense, and not confined to the tonsils, but involved the uvula, velum palati, and posterior fauces generally, and in a few instances extended even to the salivary glands, occasioning a disagreeable spontaneous ptyalism."

Assistant Surgeon B. I. D. Irwin reported the prevalence at Fort Buchanan, Arizona, in March, 1858, of "a most severe epidemic catarrh," with, "in every case, tonsillitic or pharyngeal inflammation." All of these epidemics appear to have presented some resemblance, at least, to what is now usually called diphtheria; although no mention is made of the observation of the diphtheritic exudation in the throat—of the pathognomonic importance of which it is possible that practitioners generally are now making too large an estimate.

Army service, especially at remote stations, affords excellent opportunities for the study of *scorbutus*. Many of the reports in this volume give account of it. Besides deficiency in the supply of fresh vegetable food, it is evident that other causes are promotive of scurvy; and are even capable in some instances of producing it, notwithstanding the use of articles of diet supposed to be antiscorbutic. Prominent among these causes are drunkenness, filth, despondency, ennui, and monotonous guard duty at night. The fullest investigation of the subject is presented in the reports of Assistant Surgeon E. W. Johns, at Fort Laramie, Nebraska. All of the reporters agree that potatoes are the best of the antiscorbutics; that citric acid, and even lemon juice, as well as the salts of potassa, frequently disappoint, both in prevention and cure; and that, although the "desiccated vegetables" sometimes answer an admirable purpose, yet the amount of their supply must be liberal in order to be efficient. The wild artichoke, water-cress, wild onion, cactus opuntia, agave Americana, phytolacca decandra, and various species of rumex, are named as indigenous remedies and prophylactics of ascertained value.

The anticipation entertained by some, that cholera might become *naturalized* or endemic in the valley of the Mississippi, does not appear to have been realized. In 1855, it prevailed to a serious extent at Jefferson Barracks and St. Louis Arsenal, at Fort Leavenworth, and elsewhere, in the Middle and Northern Divisions west of the Great Lakes, and in the South Interior Division east of the Lakes. But after that year no considerable number of cases was reported at any of the stations; the largest amount being ten cases and five deaths, at Fort Columbus, in New York harbour, in the fourth quarter of 1857.

In Florida, Fort Dallas is highly praised for its salubrity, by Assistant Surgeon R. F. Simpson; while a similar encomium, for equability of temperature and freedom from local causes of disease, is passed upon Fernandina and Cedar Keys, by Surgeon General Lawson, in a letter to Hon. D. L. Yulee.

Assistant Surgeon A. K. Smith, at Fort McKavett, Texas, gives some interesting information in regard to the serious effects of the bite of the *tarantula*. At Fort Buchanan, Arizona, Assistant Surgeon Irwin describes

tarantulas eight inches in length, with fangs an inch long; and centipedes ten inches long by one inch in breadth. These, as well as the vinagrilla and the scorpion, or *alacran* of Sonora, produce poisoned wounds, attended by severe suffering and even danger to life.

Dr. Irwin and several of the other contributors to the volume under notice, furnish ethnological details in regard to the native Indian tribes; whose number is still rapidly diminishing. Intemperance, syphilis, small-pox, and miasmatic fevers are their principal destroyers; the mortality among them from disease being greater than among the whites. According to Assistant Surgeon C. C. Keeney, this is especially the case with those fixed against their will on *reservations*. The latter writer notices, also, in his report, the great prevalence of *ophthalmia* in Scott's Valley, California; and the remarkable tendency to *prolificacy*, under the influence of the same climate, not only in human females but among the domestic animals.

Perhaps the most interesting portion of the work before us is that comprising the reports by Assistant Surgeons Bartholow, Moore, Ridgely, Milhau, and Brewer, of the sanitary history of the military expedition to Utah, in 1857 and 1858. In July, 1857, about seven hundred men, of the Tenth Infantry, commenced their long march of twelve hundred miles, from Fort Leavenworth, in Kansas, across the prairies to Salt Lake City, Utah Territory. Half of the command were new recruits; much of the disease occurring during the expedition being due to the broken-down constitutions or bad habits of those immature or untrained campaigners. The previously healthy, prudent, and temperate men constantly improved in physical condition during the march.

Diarrhœa, under dietetic and climatic causes, largely preponderated over all other forms of disease. Several cases of scorbutus also occurred.

In August, while passing through the Platte Valley, intermittent and remittent fevers appeared. After leaving Fort Laramie, no new cases presented themselves until they reached Ham's Fork, a stream running through an elevated valley 6,640 feet above the level of the sea. Can malaria be the product of local circumstances at such a height? Or, were the cases of periodical fever among these troops the result of the incubation of miasma introduced into the system in the Platte Valley, which they had left but ten days before? Either of these suppositions is tenable; the question as to which is the more probable, is ably discussed by the writers of these reports; although an exclusive settlement of it is hardly yet possible. Except in elevation, the valley of Ham's Fork is described by Assistant Surgeon Bartholow as resembling in all respects, in soil, temperature, and productions, the valley of the Platte River, where miasmatic fevers are rife.

The disease which is endemic in the high region alluded to, and elsewhere in the elevated country among the Rocky Mountains, has been called by some the "mountain fever." In its general character it appears to resemble the autumnal fever of the low country, with a decidedly adynamic tendency; but there are some discrepancies in the accounts given of it. Thus Dr. Ewing, in a paper in the *St. Louis Medical and Surgical Journal*, March, 1855, asserts the absence of nausea in mountain fever. Assistant Surgeon Bartholow observed constant nausea and occasional vomiting in the cases under his charge. Diarrhœa is, by the latter writer, spoken of as "in all cases a persistent symptom." Assistant Surgeon J. J. Milhau states that in those seen by him the bowels were "nearly always

constipated." The hot stage, according to Dr. Bartholow, was "in but few cases followed by a sweating stage." In the experience of Assistant Surgeon C. Brewer, "throughout the course of the malady, previous to the remissions, copious perspirations occur." Drs. Bartholow and Brewer agree as to the *irregularity* of the paroxysms and remissions or intermissions. But Dr. Milhau remarks that one of its most striking features is "the uniformity of the train of symptoms in the different cases." A similar variation of opinion exists in regard to the treatment, although all unite in asserting that the mild cases tend toward recovery in six, eight, or ten days. A relapse often takes place on the fifth or sixth day. Dr. Bartholow "had the most satisfactory evidence" of the curative power of quinine in the serious cases. The mildest he did not consider to require it. On the contrary, Dr. Milhau, having prescribed quinine, found that its action was not favourable, and abandoned its use. He believes quinine to be injurious in this disease. It was used by Dr. Brewer in the intermissions and decided remissions. Larger experience is evidently wanting, before "mountain fever" can take its definite place in our nosography. In some respects, a resemblance may be imagined between it and the "relapsing fever" of Scotland.

The following extracts from the report of Assistant Surgeon Bartholow, in regard to the physiological influences of Mormonism, will probably interest the reader:—

"Isolated in the narrow valleys of Utah, and practising the rites of a religion grossly material, of which polygamy is the main element and cohesive force, the Mormon people have arrived at a physical and mental condition, in a few years of growth, such as densely populated communities in the older parts of the world, hereditary victims of all the vices of civilization, have been ages in reaching. This condition is shown by the preponderance of female births, by the mortality in infantine life, by the large proportion of the albuminous and gelatinous types of constitution, and by the striking uniformity in facial expression and in physical conformation of the younger portion of the community."

"The activity of the reproductive function, as a rule, is not diminished by polygamy; on the contrary, the women are remarkable for fecundity; but in the harems the proportion of children arriving at maturity is much less than in the rural districts of our country."

"If Mormonism received no additions from outside sources, these influences continuing, it is not difficult to foresee that it would eventually die out, the increase of population, independently of large annual accessions from abroad, has not been coequal with the increase in other portions of our country. The results of polygamy here are not to be compared, without some limitations, to the results of the same institution elsewhere; its decadence must follow more speedily. In eastern life, where it has been a recognized institution for ages, women are prepared for its continuance, and do not feel degraded by their association with it. The women of this Territory, how fanatical and ignorant soever, recognize their wide departure from the normal standard in all Christian countries; and from the degradation of the mother follows that of the child, and physical degeneracy is not a remote consequence of moral depravity."

In the very full and carefully systematized tables and abstracts of disease and mortality, which occupy about a hundred pages of this large quarto volume, the sanitary statistician will find a vast deal of material, important especially on account of its *definiteness* of statement. We cannot avoid the expression of deep regret, that sufficient interest has not yet been awakened in the subject of medical statistics, to cause an effort, in civil practice, to obtain similar results, by the accurate registration, on a large scale, of the amount of disease as well as of mortality. H. H.

ART. XIV.—*Die Cellular Pathologie, in ihrer Begründung auf physiologische und pathologische Gewebelehre.* Von RUDOLF VIRCHOW, o.ö. Prof. der path. Anat., &c. &c. Mit 144 Holzschnitten. Zweite neudurchgesehene Auflage. Berlin, 1859.

La Pathologie Cellulare, basée sur l'étude physiologique et pathologique des Tissus. Par R. VIRCHOW. Traduit de l'allemand sur la seconde édition, par PAUL PICARD. Edition revue et corrigée par l'auteur, avec 144 figures. Paris, 1861.

Cellular Pathology, as based upon Physiological and Pathological Histology. Twenty lectures delivered in the Pathological Institute of Berlin, during the months of February, March, and April, 1858, by RUD. VIRCHOW, Public Professor in Ordinary of Pathological Anatomy, &c., in the University of Berlin, &c. &c. Translated from the second edition of the original, by FRANK CHANCE, with notes and numerous emendations, principally from MS. notes of the author, and illustrated by 144 engravings on wood. London: J. Churchill, 1860.

PERHAPS no man living at the present time enjoys a higher reputation for honesty of purpose and keenness of observation in pathological research than Professor Rudolph Virchow. The history of medical progress in this department during the last ten years and more is full of his name. To the progress which scientific medicine has achieved during this period he has contributed perhaps more than any other single individual. To ignore his papers scattered in various German journals, and especially in that of which he is the editor (*Virchow's Archiv*), would be to ignore some of the most prominent landmarks of medical advance. Full of the spirit of investigation, himself an investigator of the first ability, Virchow is remarkable among the moderns for the reverence with which he regards the past. It is one of the sins of the present of medicine to despise the past, to think of it lightly or contemptuously, to overlook completely our own deficiencies in our inordinate perception of the deficiencies of our predecessors. This is an error as hurtful and not so noble as the blind adoration of the ancients, so common fifty years ago. In this Virchow is conservative.

"I have already often been reproached," he says, "with endeavouring to rehabilitate antiquated views in modern science. In respect to this, I can, I think, say, with a safe conscience, that I am just as little inclined to restore Galen and Paracelsus to the position they formerly held, as I am afraid openly to acknowledge whatever truth there is in their views and observations."—*Preface to first edition.*

The *Cellular Pathology* consists of twenty lectures which were originally delivered in 1858 to a considerable number of the physicians of Berlin, and which were subsequently published from short-hand notes taken for the purpose. A second edition soon followed, in which, however, the author limited himself to "improving the language, expressing in more precise terms what was liable to be misunderstood, and expunging repetitions." (*Pref. to 2d edit.*) The second edition was speedily followed by French and English translations, both of which were made under the supervision of the author.

These lectures created a sensation. We are told by Paul Picard, the French translator, that "all the physicians who followed his course, carefully examined his preparations, and seriously heard his lectures, were con-

vinced." In their printed form they have already been circulated extensively throughout Europe, and have everywhere been received with respect, even by those who still dissent from many of the views expressed.

Perhaps many of our readers, however, will be surprised when we tell them that the *Cellular Pathology* contains very little that is new. Even those facts and views which more particularly belong to Virchow have for the most part been published by him elsewhere. Its author acknowledges this in the most modest manner.

"I trust, therefore, that what I offer," he says, "may not be taken for more than it is intended to be. Those who have found leisure enough to keep up their knowledge by reading the current medical literature will find but little that is new in these lectures."—*Preface to first edition.*

This fact, however, in nowise impairs—on the contrary, it in every way increases—the value of the work; it may be regarded as rendering accessible to the general medical reader not merely the richest fruits of the twenty years' scientific labours of its distinguished author, but also as a reflex of the progress of the whole German mind in this direction during the decennium which has just expired. For the doctrines of the *Cellular Pathology* are not the product of any one single mind alone. Step by step, almost from the very day of the publication of Schwann's researches, such investigators as Hugo von Mohl, Remak, Reichert, and others, as well as Virchow himself, have collected the materials without which such generalizations as these would have been impossible.

The fundamental idea which underlies the cellular pathology is a total denial of the possibility of the spontaneous generation of cells in a blastema, or, as Schwann's disciples have called it, "free cell-development."

"Even in pathology we can now go so far as to establish as a general principle *that no development of any kind begins* de novo, *and consequently as to reject the theory of equivocal* (spontaneous) *generation just as much in the history of the development of individual parts as we do in that of entire organisms.* Just as little as we can now admit that a tænia can arise out of saburral mucus, or that out of the residue of the decomposition of animal or vegetable matter an infusorial animalcule, a fungus, or an alga can be formed, equally little are we disposed to concede, either in physiological or pathological histology, that a new cell can build itself up out of any non-cellular substance. Where a cell arises, there a cell must have previously existed (*omnis cellula e cellula*), just as an animal can spring only from an animal, a plant only from a plant."

And again:—

"An eternal law of *continuous development* prevails."—p. 27.

How fundamentally these views are opposed to the notions generally received of the formation of pathological tissues will be best appreciated by comparing the following quotation from the Sydenham translation of Kölliker's *Microscopical Anatomy,* which well represents the view at present received as orthodox by the majority of American physicians:—

"Free cell-formation is exceedingly frequent in pathological productions, and the cells in pus and in exudations of all kinds arise in this manner; in fact, all pathological cell-formation properly comes under this head."

This, however, was Kölliker's opinion for pathological formations only; with regard to the *physiological* production of tissues he writes, in the same page:—

"With regard to physiological processes, as has been already shown, free cell-development has been much too readily taken for granted."

And again:—

"Botany knows no free cell-development."[1]

In order to arrive at a correct appreciation of these contradictory dogmas, it is necessary to glance briefly at the historical evolution of the theory of cell-development. The "cell theory" may be regarded as having originated, in very nearly its present form, with the distinguished botanist Schleiden, whose investigations on this subject were given to the world in 1837.[2] These researches referred to the development of *vegetable cells*. Schwann's investigations (*Mikroskopische Untersuchungen*), published the two following years, had for their object, by an investigation of the animal tissues, and especially of the tissues of the embryo, to render Schleiden's doctrines applicable to the animal world. Schwann started upon his investigation from Schleiden's stand-point, full of the preconceptions that would naturally spring from such a source. In fact, therefore, it is to Schleiden's researches that we are to look for the data on which the theory of free cell-development is to be based.

But although Schleiden and Schwann, by their publications, have given a great impulse to minute investigation, and have added to the general stock of our knowledge innumerable details, we cannot regard either of them as deserving the credit, usually ascribed to them, of originating the idea that all animal and vegetable tissues, even the most dissimilar, originate out of cells. In this idea they were more or less completely anticipated by others, and, above all, by Raspail, the father of micro-chemistry, a man whose labours in this direction have been too generally ignored. In Raspail's *New System of Organic Chemistry*, the first edition of which, in one volume octavo, was published in Paris in 1833, four years before the publication of Schleiden's researches, will be found the germ of the more elaborate views of Schleiden and Schwann.

"In the course of this work," says Raspail (for the convenience of the reader, the quotation is from Henderson's English translation, London, 1834), "we shall more than once have occasion to prove that the type of organized beings may be reduced in its simplest expression to an imperforated vesicle, possessed of the property of elaborating for its own nourishment, to an indefinite extent, the gaseous or liquid substances which it takes in by aspiration, and of throwing off by expiration such of their elements as are incapable of being assimilated."

These vesicles Raspail described as originating in "a liquid tending to organize," in which we at once recognize the cytoblastema of Schwann. He even compares the process of their formation to crystallization, a comparison elaborated by Schwann, who has been generally believed to have originated it. Raspail himself, however, by no means observed *de novo* even the large majority of the microscopical facts on which his vesicular theory reposed. The isolated microscopical observations of the eighteenth century had been gradually accumulating since the beginning of the nineteenth, and even systematic attempts had been made at classification, notable among which may be noticed that of Treviranus in 1816, and of Heusinger in 1824.

Although, however, Schleiden's views were thus far anticipated by Raspail in their general results, the special observations upon which he relied for their support were original and valuable. Schleiden, we have already said, was a botanist; the situations in which he studied cell-development

[1] Kölliker's Mic. Anat., Amer. edit., p. 49.
[2] Untersuchungen über Phytogenesis, Müller's Archiv, 1838.

were especially the large cell (embryonal sac) which subsequently contains the so-called albumen of the seed, and the extremity of the pollen-tube from which the embryo itself is developed.[1] According to him, in both these situations there exists a solution of sugar or gum, which at first becomes cloudy from the formation of innumerable granules; then, by the aggregation of the granules, nucleoli are produced; around these are formed the nuclei (cytoblasts), and around these the cells. When, however, Schleiden endeavoured to extend these observations to other parts of the plant which are the seat of active growth, he found the observations full of difficulty. Especially does he acknowledge his inability to make out the process in that remarkable layer (cambium) beneath the bark in which proceeds the growth of exogenous plants in thickness. It will at once be seen that these observations of Schleiden demonstrate, if they be accurate in all particulars, the formation of cells within cells, but do not in themselves justify in any way the idea of the free development of cells in a formative liquid outside of cells. Schleiden himself perceived this; and in summing up the probable deductions from his observations, we find him, therefore, saying:—

"If we have found the growth of the embryo to consist only in the *formation of cells within cells*, we *may expect* to find the same result also in the process of growth of the whole plant."[2]

And again:—

"The entire growth of the plant consists only of *a formation of cells within cells*."[3]

Turning now from Schleiden to Schwann, we are at once struck with the fact that while this observer traces out with considerable accuracy, and with perhaps fewer errors of interpretation than might have been expected in a new field of investigation, the process by which various tissues are produced out of cells, he, nevertheless, mainly relies for his account of the fashion in which *cells* arise upon Schleiden. But Schwann went further than Schleiden; finding innumerable cells in the embryonic tissues, lying in a semi-solid, more or less transparent substance, of various consistency, he too hastily concluded that this had acted as the blastema or formative substance out of which the cells had been generated by a process similar to what Schleiden had described as going on *within* cells. Schleiden had taught what is now called endogenous cell-formation; Schwann went further, and taught free cell-formation.

No one can read Schwann's book carefully without being at once convinced that this notion, though rendered exceedingly plausible by his ingenious reasoning, is based upon a very slender stock of facts; in truth, the whole strength and value of the work, and of the vast mass of its original observations, is to be found in connection with the processes by which the cells, once produced, are variously transformed; for the initial production of the cells he acknowledges his reliance upon the observations of Schleiden.

The doctrines which Schwann had announced for normal tissue genesis were speedily applied to pathological new formations by Johannes Müller.[4] The work of Müller constitutes an era in the history of pathology. To him belongs the credit of having placed the classification of pathological

[1] Contributions to Phytogenesis, Sydenham translation, p. 237.
[2] Loc. cit., p. 253.
[3] Loc. cit., p. 257.
[4] Bau der krankhaften Geschwülste.

products, and especially of morbid growths, upon a rational basis. To him, also, we owe the enunciation of that fundamental law of the similarity between pathological and normal tissue development which has since been so triumphantly established by Wedl and Virchow. But while Müller's remarkable work fully deserves this high praise, we are obliged to censure him for having too blindly and without sufficient evidence received Schwann's doctrine of free cell-development. In fact, Müller's own observations are chiefly connected with the processes by which the several pathological products are developed out of cells, while for the formation of these he mainly relies upon Schwann.

Müller's pathological researches were soon followed by those of Vogel, who also, with the majority of pathologists—as Lebert, Rokitansky, Paget, and others—accepted Schwann's views of cell-development.

But while the pathologists were thus contented with the doctrine they had received from the botanists and anatomists, these were revising their labours, and variously modifying their doctrines, as the stock of facts was increased by observation.

It would not be possible, within the limits we have assigned ourselves, to describe step by step the progress made in these particulars. From the time of Schwann, observers multiplied. Indeed, it is the highest praise of his labours, that they were well calculated to stimulate the observations of others. A bare enumeration of the names of these investigators would be but a barren task. To specify the labours, and define the scientific position and value of each, to sift from the mass of the writings of every one the golden grains of truth—the *new facts*—which each contributed to the common stock, would fill a moderate-sized volume. It is only here possible to allude to one or two prominent names. And thus, among the botanists, honorable mention should be made of Hugo von Mohl, whose memoirs will be found in the *Linnæa*, the *Botanische Zeitung*, &c. &c. (or translated in the *Annales des Sciences Naturelles*, the *Annals and Magazine of Natural History*, and Taylor's *Scientific Memoirs*). This writer, whose earlier observations almost immediately followed those of Schwann (see *Botanische Zeitung*, vol. ii., 1844), arrived at conclusions differing from his in a remarkable manner. Not only did he deny free cell-development, affirming that "the formation of cells in plants occurs only in the cavities of older cells,"[1] but even in his account of the method in which cells arise within other cells, he differs materially from Schwann and Schleiden; thus, for example, he denies that the formation of the nucleolus precedes that of the nucleus.

So, also, among the anatomists, Henle, Mandl, and Reichert raised doubts, in animal cell-development, as to the pre-existence of the nucleolus, and as to other details of Schwann's doctrine, until at length, in 1852, Remak, in a valuable paper, denied in toto free cell-development, and announced, as his fundamental conception of cell-growth, the doctrine "*omnis cellula in cellula.*"[2] A doctrine which was reiterated by Virchow in his former great work,[3] in almost the very words—"*omnis cellula e cellula*"—and which has become the basis of the cellular pathology.

We have referred to these historical points, at the risk of being thought

[1] Die Vegetabilische Zelle, in Wagner's Handwörterbuch, vol. iv. p. 211.

[2] Remak, Ueber extra cellulare Entstehung thierischen Zellen, u. die Vermehrung derselben durch Theilung, u. über Entstehung des Bendegewebes, u. d. Knorpel. Müller's Archiv., 1852.

[3] Virchow, Beitr. z. Spec. Pathologie und Therapie, 1854.

tedious, because the rejection of free cell-development, if correct, must necessitate a thorough pathological reform; for the humoral pathology of Rokitansky and the Vienna school, which has been so generally received in Europe and America, falls with Schwann's doctrine; and, therefore, although no doubt the foregoing points will be familiar to many readers, we have presented them, with the desire to impress upon those who are not so well acquainted with the literature of the subject, the fact that Virchow is not to be regarded as a reckless innovator, whose propositions rest only upon his own *ipse dixit*, but that his leading doctrines are the legitimate fruit of the labours of many investigators, and that probably at the present moment, however much they might differ about minor matters, the majority of the investigators in this direction will heartily indorse the doctrine "omnis cellula e cellula."

The medical reader will at once inquire—if this be so, if free cell-development is to be rejected, how shall we account for the various pathological products (lymph, pus, tumours, cancer, tubercle, &c.) which have been supposed to be evolved by this process in the blastema or plasma exuded from the vessels of the diseased part? To this, Virchow at once replies:—

"We may, therefore, with trifling restrictions, *substitute for the plastic lymph—the blastema of the earlier, the exudation of the later, writers—connective tissue with its equivalents as the common stock of germs of the body*, and directly trace to it, as the general source, the development of new formations." (p. 398.)

The rôle played, in Virchow's comprehension of pathological processes, by connective tissue is, it will be perceived, in marked contrast with those entertained of this tissue up to the present time by very many physiologists, who have looked upon connective tissue as a very passive structure, having purely mechanical functions, and but little liable to the diverse morbid processes.

To understand Virchow's views of the genesis of pathological formations, it will be necessary to refer to his ideas of the structure of normal connective tissue, which differ in many respects from those ordinarily received in this country. The dominant view with us is, that both the white fibrous and the yellow elastic elements, which, variously intermixed, constitute this tissue, are developed *out of cells;* and especially with regard to white fibrous tissue is it believed that the renowned bundles are developed out of cells which elongate, become spindle-shaped, finally fibrillate, and thus produce *the bundles*, which are to be regarded, therefore, as transformed cells. According to Virchow, on the contrary, the white fibrous bundles do not represent transformed cells, but are produced by the direct fibrillization or transformation of the intercellular substance, which Schwann regarded as the blastema out of which the imbedded cells were developed.

As with his negation of free cell-development, so here, Virchow does not stand alone. As early as 1843, L. Mandl[1] criticized Schawnn's comprehension of connective tissue, and expressed the opinion, based upon his own investigations, that the white fibrous tissue is produced by the direct transformation of the intercellular material. In fact, from the very date of Schwann's work, connective tissue was made the object of a host of investigations; and the labours of Henle, Reichert, and Remak paved the way for the more complete view of Virchow.

While the intercellular substance is being transformed into a more or less distinctly fibrillated material, some of the cells are developed into elastic fibres, others into fat or pigment cells; but a large proportion of them,

[1] Manuel d'Anatomie Générale. L. Mandl, Paris, 1843.

having assumed a spindle shape, or sometimes a more or less stellate form, abide as permanent elements in the connective tissue of the body. These stellate—frequently anastomosing—minute elements it is which serve as the germ for the majority of pathological productions, and it is to them that Virchow applies the designation of "*connective tissue corpuscles.*"

As may be supposed by what has been said, a large part of "The Cellular Pathology" is occupied by descriptions of processes and transformations of a purely physiological kind, but which, nevertheless, must be comprehended in order to appreciate the new doctrines. Passing by these points, let us glance at the new doctrines with regard to some of the most important pathological processes.

And first, of irritation and inflammation.

Irritability is, according to Virchow, the property which every living cell or cell derivative possesses of being compelled into action, by influences coming from exterior sources. It is the very criterion of life.

"Vital activity is, as far at least as we are able to judge, nowhere, in no part whatever, carried on by means allotted to it from the very beginning, and entirely confined to it, but we everywhere see that a certain excitation is necessary for its production" (p. 286); and as, whenever "a given action is called into play, we have to deal with a manifestation either of the *function*, the *nutrition*, or the *formation* of a part" (p. 288),

he discriminates between *functional*, *nutritive*, and *formative* irritability; which, nevertheless, may complicate each other.

Functional irritability is displayed when, in consequence of some influence exterior to itself, a part performs its function, a muscle contracts, a gland secretes, &c. It is evidently, in its nature, a purely physiological phenomenon, and only becomes morbid when excessive. The stimuli capable of calling into activity the functions of particular parts are not indifferent. "Nearly everywhere do we find *specific relations* or *affinities* to exist." (p. 293.)

Nutritive irritability manifests itself when, in consequence of some stimulus, the nutritive activity of parts is augmented. By Virchow, the term is limited to the conditions in which there is an increase in the size of the individual cells of the part affected; they take into their interior an unwonted quantity of nutritive material, and thus increase in dimensions while their contents becomes turbid. Thus—

"If we draw a thread through a cartilage, so that merely a traumatic irritation is produced, we see that all the cells which lie close to the thread become enlarged through an increased absorption of material." (p. 298.)

Such an enlargement and turbidity of the cells of the part, without other change, constitutes a frequent form of corneal opacity (parenchymatous keratitis). (p. 301.)

Formative irritability is discriminated from nutritive by resulting in an increase in the number of elements.

"If, namely, we follow up the higher degrees of irritation which take place in a part, we find that the cellular elements, shortly after they have experienced the nutritive enlargement, exhibit further changes which begin in the interior of the nuclei, generally in such a manner that the nucleoli become unusually large, in many cases somewhat oblong and sometimes staff-shaped. Then, as the next stage, we usually see that the nucleoli become constricted in the middle, and assume the form of a finger-biscuit; and, a little later, two nucleoli are found." "About such a divided nucleolus, the finger-biscuit-like constriction, and afterwards the real division of the nucleus takes place." "This may be repeated so

that three, four, or more nuclei arise." (pp. 306–7.) "If, now, we advance a step farther in these processes, we come to the new formation of the cells themselves." (p. 310.)

The cell most frequently, but not always, parts so soon as two nuclei are formed, two new cells resulting; and this process may be repeated until large groups of cells have thus been produced from single ones.

Each of the above forms of irritation may coexist with vascular or nervous disturbance; but these are collateral or consequential, as the case may be. They are not, as they have been so generally believed to be, the starting-point of the processes; for they may be manifested in nerveless and non-vascular parts (cornea, cartilage, &c.); and in parts supplied by both vessels and nerves, the phenomena manifest themselves, not in the track of these, but in the track of the irritant.

"If we make the experiment with the thread upon the skin, a whole series of nerve territories are intersected by it. Still, the whole of the territories belonging to the nerves which lie along the thread are not thrown into the same morbid condition, but the nutritive irritation is limited to the immediate vicinity of the thread." (p. 299.)

The two latter of these forms of irritation play an important part in the phenomena of *inflammation*.

Virchow describes "two forms of inflammation which can be separated from one another: the *purely parenchymatous inflammation*, where the process runs its course in the interior of the tissue, without our being able to detect the presence of any free fluid which has escaped from the blood; and the *secretory* (*exudative*) *inflammation* (which belongs to the more superficial organs), where an increased escape of fluid takes place from the blood, and conveys the peculiar parenchymatous matters along with it to the surface of the organs. That there are two different forms, is clearly shown by the fact that they occur, for the most part, in different organs." (p. 393.)

With regard to secretory or exudative inflammation, this may be accompanied by the accumulation, upon surfaces or in cavities, of *fibrin*, or by the production of mucus upon the mucous membranes. Now just as, "although a mucous membrane produces incredibly large masses of mucus in a short time, they are, nevertheless, products of the membrane itself," "the peculiar mucin matter" being "a product of the membrane," "conveyed to the surface by means of the fluid oozing through from the blood;" so, also, instead of regarding the fibrin (coagulable lymph) "as a real transudation from the liquor sanguinis, as the outflowing plasma, I have proposed the explanation that the fibrin, like the mucus, is a local product of those tissues on and in which it is found, and that it is conveyed to the surface in the same way as the mucus of the mucous membrane." (p. 392.) Absorption of the fibrin, thus locally produced, into the blood, is regarded as the cause of the increased quantity of fibrin observed in the blood in inflammatory fever. "The fibrinous crasis is just as much a product of the local disease, as the fibrinous exudation is a product of the local metamorphosis of matter." (p. 392.) The cellular elements (lymph, pus, mucous corpuscles, &c.) observed in the fibrin or mucus, are not new formations in the blastema afforded by the exudation, but are produced out of the cells of the parts involved, by the powers above described under the head of formative irritability.

In parenchymatous inflammation, on the other hand, there is nothing withdrawn from the bloodvessels which does not accumulate *in* the cells of the parts involved; these increase in size, become turbid, and even division

of nuclei and production of new elements may take place, or at any stage of the process various degenerative changes may set in (*e. g.*, fatty degeneration). This form of inflammation is especially observed in the liver, kidneys, and other parenchymatous organs.

It will thus be seen that for Virchow the starting-point of an inflammation, as well as an irritation, is to be found not in the bloodvessels, nor yet in the nerves, but in the *cells of the part affected.*

"If now, in the next place, we investigate the history of SUPPURATION, we immediately discover that we must distinguish two different modes of pus-formation, according, namely, as pus proceeds from tissues of the first two kinds mentioned in our classification—*i. e.* from *epithelium* or from *connective tissue.*" (p. 445.)

In the first instance the pus-cells are produced by the multiplication of the cells of the younger strata of the *epithelium* (*e. g.* the rete Malpighi for the skin).

"The more deeply-seated pus-formation regularly takes place in the *connective tissue.* In it there first occurs an enlargement of the cells (connective tissue corpuscles); the nuclei divide, and for some time multiply excessively. This first stage is then very soon followed by divisions of the cells themselves. Round about the irritated parts, where before single cells lay, pairs or groups of cells are subsequently found, out of which a new formation of an homologous kind (connective tissue) usually constructs itself. More in the interior, on the contrary, where the cells were early abundantly filled with nuclei, heaps of little cells soon appear, which at first still preserve the direction and forms of the previous connective tissue corpuscles. Somewhat later we find here roundish collections, or diffuse 'infiltrations,' in which the intermediate tissue is extremely scanty, and continually liquefies more and more in proportion as the proliferation of the cells extends." (p. 451.)

After the abscess produced by such a process discharges, it is healed by granulations, for the production of which it is only necessary that the intercellular substance should retain a certain degree of consistence. So that the essence of the diversity between the formation of pus and the formation of granulations consists in this—that where suppuration is going on, the degenerative processes predominate over the formative, causing the intercellular substance to liquefy, and depriving the cells of further formative capacity; while in the case of granulations the formative processes predominate.

By a similar cell-multiplication in connective tissue are those granulations produced by which injuries are repaired.

If these views are admitted, it is no longer necessary to attribute to pus a corrosive action in order to account for the phenomena of *ulceration.*

"*Pus is not the dissolving, but the dissolved*—i. e. *the transformed tissue.*" (p. 446.) Wherever new elements are produced, they are produced *out of old ones, which cease to exist in this very act.* If, then, as in a progressing ulcer, the product of the cell-multiplication be constantly pus, a solution of continuity must result, and the ulcer will continue to increase in size so long as this transformation continues in excess.

If, now, from the inflammatory processes we turn to the production of morbid growths, tumours innocent or malignant, we find the same general principle maintained by Virchow. Everywhere the elements of the new growth arise out of the cells of the involved tissue, and especially out of the connective tissue corpuscles, by a process quite like that given above for pus and for granulations. If this statement be true, we need not be surprised to find asserted as its legitimate corollary—

"*There is a stage when it is impossible to decide with certainty whether we have in a part to deal with simple processes of growth, or with the development of a hetero-plastic, destructive form.*" (p. 454.)

And again:—

"The first development of cancer, of cancroid, and of sarcoma, exhibits the same stages; if the course of their development be traced sufficiently far back, we at last always come across a stage in which, in the younger and deeper layers, indifferent cells are met with, which do not until a later period, according to the particular nature of the irritation to which they are exposed, assume the one or the other type." (p. 454.)

And here, in fact, is the explanation of the thousand and one errors of diagnosis which have been made by over-confident microscopists, and, above all, in the differential diagnosis of cancer.

In the classification of the several new formations, Virchow still preserves the division into *heterologous* and *homologous*. But he uses these terms in a very different sense from that in which they are commonly employed by those who apply the term heterologous only to the elements of cancer and tubercle.

"Heterologous we may call not only malignant, degenerative neoplasms, but we may also thus designate every tissue which deviates from the recognized type of the part; whilst we should call all that homologous which, though new-formed, still reproduces the type of its parent soil." (p. 442.)

Thus, for Virchow, the cells of pus, even the cells contained in the catarrhal mucus of an inflamed mucous membrane, are heterologous forms, because they are unlike the normal cells by the proliferation of which they were produced. At the same time, he distinctly proclaims—

"*Heterologous tissues also have physiological types.*" (p. 480.)

Perhaps the following passage best explains Virchow's views on this mooted question of heterology:—

"But if we ultimately arrive at such a simple view of the matter, the question of course arises, What becomes of the doctrine of the *heterology* of morbid products, to the upholding of which we have long been accustomed, and to which the most simple reflection almost inevitably conducts us? Hereunto I can return no other answer than that there is no other kind of heterology in morbid structures than the abnormal manner in which they arise, and that this abnormity consists either in the production of a structure at a point where it has no business, or at a time when it ought not to be produced, or to an extent which is at variance with the typical formation of the body." "But we must be very careful not to connect this kind of heterology, in the more extended sense of the word, with the notion of *malignity*." (p. 64.)

On the question of *malignity*, views are expressed very similar to those advocated by Bennet in his *Clinical Medicine*.

"Cancer is not malignant because it contains heterologous cells, nor cancroid benignant because its cells are homologous; they are both malignant, and their malignity differs only in degree." (p. 484.) Thus, "Cartilaginous tumours (enchondromata), which were formerly described as unquestionably benignant, sometimes occur in soft and rather gelatinous forms, which may occasion just such internal metastases as cancer, properly so called." (p. 486.)

So sarcomata, under certain circumstances, may "appear secondarily in the lymphatic glands, and in many cases occur throughout the whole body metastatically to such an extent that scarcely any organ is spared by them." (p. 487.)

If, now, we investigate the method by which morbid growths infect distant organs, we shall find that Virchow admits two distinct methods: either, on the one hand, the cellular elements of the growth may pass into the blood, as occasionally, but probably with great rarity, happens; or, on the other, contaminating juices originating in the focus of the disease, containing, however, no organic forms, may sweep into the lymphatic or venous circulation, and excite in distant parts the same action as existed at the seat of the primary disease.

The doctrine of a special blood disease preceding the first tumour is denied, as resting on insufficient data, a criticism which is certainly just, but which applies with equal force, in the present state of the investigation, to the view proposed by Virchow as a substitute.

The recurrence of certain growths after extirpation, however, is much more logically and satisfactorily accounted for by the interesting statement that in such cases—

"*The zone produced at the latest period of the disease extends to a considerable distance beyond the zone of degeneration that can be discerned by the naked eye.* If we examine any proliferating tumour of a cellular character, we often find, three to five lines beyond its apparent limits, the tissues already in a state of disease, and exhibiting the first traces of a new zone. This is the chief source of local recurrence after extirpation, for it proceeds from the zone that cannot be detected with the naked eye, beginning to grow in consequence of the increased supply of nutritive material which results from the removal of the original tumour. No new deposit from the blood takes place there, but the new-formed germs, which already lie in the neighbouring tissue, run through their further development in the same manner that it would otherwise have taken place, or perhaps even still more quickly." (p. 458.)

Notwithstanding the similarity of the conditions presented in the early stages of the most diverse new formations, such evident diversities are established sooner or later, that *Virchow is of the opinion that a classification of tumours should be based upon their microscopical anatomy*, and not upon their coarser external features alone; but he cautions especially against haste in inventing new names by which we may render "things which have long been known strange to the mind of people in general."

Such are the leading doctrines of the *Cellular Pathology* with regard to the genesis and relations of all the so-called new formations; but, besides the doctrine of new formation, many other themes are discussed in these lectures: the classification of physiological tissues; a *résumé* of the latest discoveries with regard to their minute anatomy; the doctrine of nutrition; the minute structure, the physiological and pathological relations of the nervous system; fatty, amyloid, and other degenerations; leukæmia, pyæmia, metastatical dyscrasiæ, &c. It is not possible to present even the briefest abstract of most of these subjects; and, indeed, the object of this review was chiefly to call attention to the new views of the genesis of morbid products. But an account of this book would be most incomplete should it pass by in silence the views expressed with regard to tubercle, leukæmia, and the doctrine of thrombi and emboli.

The designation TUBERCLE is limited by Virchow to the miliary form. He includes under this head the tubercles of the arachnoid, which, as is well known, were excluded from the category of tubercle by Robin because they contained cells. In fact, however, for Virchow, there is a period in its history when every tubercle contains cells. For tubercle generally, like other new formations, has its origin in connective tissue, by a process of cell-proliferation very similar to that above described for pus, and differing

mainly in the circumstance that the cells and nuclei of a young forming tubercle are smaller than those seen in the case of pus, just as those of cancer or sarcoma are larger. By and by fatty degeneration sets in in the centre of the tubercle, then complete cheesy metamorphosis.

Tubercles, such as above described, never attain any great size; the large tubercles, of the size of a cherry, a walnut, or even larger, are not simple tubercles.

"You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple" (a Borsdorf apple, 1½ to 1¾ inch in diameter), "or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them, which enlarges, not by the growth of the original focus, but rather by the continual formation and adjunction of new foci at its circumference. If we examine one of these perfectly yellowish-white, dry, cheesy tubera, we find immediately surrounding it a soft vascular layer which marks it off from the adjoining cerebral substance—a closely investing areola of connective tissue and vessels. In this layer lie the small young granules" (miliary tubercles), "now in greater, now in less number. They establish themselves externally, and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy." (p. 478.)

But the cheesy metamorphosis is not peculiar to tubercle; pus may undergo a cheesy metamorphosis, and the same may befall cancer or sarcoma. In each of these cases a product is ultimately attained which is quite one in its coarser and minute characters, no matter what its origin. It is in the cheesy matter thus produced that we are especially to look for the so-called tubercle-corpuscles, rendered so famous by the researches of Lebert. Virchow, however, by no means regards these corpuscles as immature or imperfectly-formed elements, "the first bungling products, unfortunate essays of organization;" everywhere, he believes, the tubercle-corpuscles are formed out of pre-existing elements by a shrivelling and fatty degeneration.

"You may with certainty assume that, where you meet with a largish corpuscle of this description, a cell had previously existed; and where you find a small one, there once had been a nucleus inclosed perhaps within a cell." (p. 473.)

When, then, tubercle-corpuscles occur as the product of the cheesy metamorphosis in cancer, they represent degenerated and shrivelled cancer-cells and nuclei; when they occur in the products of the cheesy transformation of pus, they represent degenerated and shrivelled pus-cells. In fact, according to Virchow, those tubercular infiltrations of the lungs, regarded as so characteristic, are, as the rule, not tubercular, but represent inflammatory products which have undergone the cheesy metamorphosis.

With this brief notice of the subject of tubercle, let us next look at our author's views with regard to leukæmia and leucocytosis.

LEUCOCYTOSIS is the name given by Virchow to the many transitory conditions in which the number of the white corpuscles of the blood is considerably increased. A similar increase in their number, of a more permanent character, is designated Leukæmia. Leucocytosis is very frequently associated with inflammation, and above all with those diffuse phlegmonous or erysipelatous inflammations in which the lymphatic glands are involved. In fact, it is "very rarely that a considerable increase of fibrin takes place without a simultaneous increase in the colourless blood-corpuscles." (p. 166.) But, on the other hand, a number of conditions may occur, and especially the typhoid processes give us examples, in which leucocytosis coexists with more or less deficiency of fibrin. These latter cases invariably present greater

or less enlargement of the lymphatic glands (*e. g.*, the mesenteric glands in typhoid fever), or of the spleen; conditions which appear to stand in a genetic relation to the increased quantity of white corpuscles. These conditions are more or less transitory, the leucocytosis disappearing on convalescence from the condition in which it took its origin. Pregnancy, long known to be accompanied by a physiological increase in the quantity of the fibrin of the blood, is also accompanied by a physiological leucocytosis. At full term, the colourless corpuscles are so abundant that, in the defibrinated blood, they form a pus-like sediment. And in certain cases of puerperal fever, this sediment has actually been supposed to demonstrate the existence of pyæmia.

LEUKÆMIA, while it agrees with leucocytosis in being characterized by an increased number of white corpuscles, is a much more serious condition. More chronic in its course, it appears to tend almost invariably to a fatal termination; of all the known cases, but one left the hospital improved; "in all the other cases, the result was death." (p. 169.)

"These cases possess, in addition, the remarkable peculiarity that, usually towards the close of life, a genuine *hemorrhagic diathesis* is developed, and hemorrhages ensue, which occur with especial frequency in the nasal cavity (under the form of exhausting epistaxis), but may also, under certain circumstances, take place in other parts of the body; as, for example, on a very large scale, in the form of apoplectic clots in the brain, or of melæna in the intestinal canal." (p. 169.)

Leukæmia is not associated, as leucocytosis often is, with increase of the blood fibrin.

Two diverse forms may be discriminated: an ordinary splenic form, associated with chronic enlargement of the spleen; and a lymphatic form, associated with enlargements of the lymphatic glands. The two forms may often coexist.

Leukæmia, it will thus be perceived, is a "permanent progressive leucocytosis"—a serious, if not fatal, chronic disease; while leucocytosis is a transitory affection, the import of which will vary with the cause on which it happens to be dependent in any individual case.

The last subject to which we shall invite attention, is Virchow's doctrine of *thrombi* and *emboli;* a doctrine by which a new light is thrown upon the subjects of phlebitis, pyæmia, and metastatic abscesses.

The term *thrombus* is applied by Virchow to a coagulum of blood formed during life within a vessel, and most frequently within a vein. The whole process which results in such formations is designated thrombosis. Such coagula were first accurately described by Cruveilhier, and were made by him the basis of the somewhat famous, but now extinct, doctrine of capillary phlebitis as the primal cause of all inflammations. The clot thus formed does not long remain unchanged. Sooner or later, a central degeneration is set up, the fibrin breaks down and becomes granular, the red corpuscles lose their colouring matter, become granular on the edges, and finally almost entirely disappear; the white corpuscles, more or less altered, are set free in the whitish or yellowish-white puriform liquid which results.

This process has been erroneously described as suppurative phlebitis; but it is evidently not suppurative, as no pus is formed, but only a fluid having some of the external characters of pus; and careful examination shows that it has no necessary connection with phlebitis, inasmuch as it occurs with great frequency in vessels, the walls of which present no trace of inflammation. In fact, Virchow utterly denies the ordinary notions of arte-

ritis or phlebitis, in accordance with which the inflammatory products were regarded as being poured out from the walls of the inflamed vessel into its lumen; on the contrary, he strenuously asserts, and in this we believe he is correct, that in phlebitis the exudations, as they are called—that is, the inflammatory products—are formed in the very thickness of the inflamed vascular wall itself, and not in the calibre of the vessel. Thrombi, such as have been above described, may coexist with phlebitis; but they may exist without it; and, on the contrary, inflammation of the vascular walls "may unquestionably exist when the current of blood within the vessels of the affected part is perfectly free and unobstructed." (p. 199.)

The thrombus frequently is limited to the vessel in which it originates, extending as far as the next larger trunk into which the involved vessel empties; but very often layer after layer of coagulum is deposited upon its extremity, encroaching upon the larger trunk, so that "the thrombus is prolonged beyond the mouth of the branch into the trunk, in the direction of the current of the blood." (p. 205.) Portions of this prolonged thrombus, detached by the action of the blood-stream of the vessel into which it projects, and hurried away into the torrent of the circulation, constitute *emboli*.

An embolus, or floating coagulum, thus set free will, if formed in one of the veins of the extremities, be readily carried along from vein to vein till it reaches the heart, and, thence proceeding to the lungs, will be arrested, in accordance with its size, in the larger or smaller branches of the pulmonary artery. Similar emboli produced in the branches of mesenteric and other abdominal veins, carried readily along to the portal vein, will be arrested in the liver. An embolus thus arrested in a small arterial twig, cuts off the nutritive supply from a certain portion of tissue; various changes may supervene, varying with the size of the vessel involved, the original condition of the thrombus from which the embolus, in any given case, was detached, &c. It is sufficient, however, for our present purposes to state that we have here one mode of the origin of those metastatic abscesses which have been supposed to denote purulent infection of the blood (pyæmia). Other metastases take their origin in endocarditis. "Ulceration takes place in one of the valves of the heart, not by means of the formation of pus, but in consequence of an acute or chronic softening; crumbling fragments of the surface of the valve are borne away by the stream of blood, and reach with it far distant points" (p. 208); as, for example, the spleen or the kidneys.

The idea that such metastases may originate in the introduction of *pus*, as such, into the blood, and its transportation from one point to another, is stoutly denied by Virchow; who, after showing the difficulties in the way of an absorption of pus, as such, asserts that the arguments in favour of the existence of a morphological pyæmia will not withstand even *gentle* criticism. Nevertheless, however, he admits another form of metastasis besides that depending on embolism; that, to wit, in which, from the primary seat of the disease, "corrupted ichorous juices," unrecognizable by our microscopes or by our chemical tests, enter the stream of the circulation and seize upon the parts for which, like certain medicines and poisons, they have a special affinity.

"To this class belongs that metastatic pleurisy which develops itself without any metastatic abscesses in the lungs; that seemingly rheumatic articular affection in which no distinct deposit is found in the joints; that diffuse gangrenous inflammation of the subcutaneous connective tissue which cannot well be accounted for, unless we suppose a more chemical mode of infection." (p. 215.)

In plain language, besides the cases of metastases which can be accounted for by Virchow's doctrine of emboli—which is so beautiful and ingenious, because it is founded upon observation—we have another category of cases which cannot be accounted for satisfactorily by any *facts* at present in our possession, and in which if, for the present, with Virchow, we resort to *suppositions*, it must be with the distinct understanding that they are only temporary makeshifts, to be flung unceremoniously away so soon as our stock of facts becomes competent to supply us with better explanations.

In the foregoing account of some of the leading views of "the cellular pathology, we have purposely abstained from comment; our intention being rather to present these views to the notice of our readers than to enter into a discussion of their merits. Into such a discussion, we are far from being inclined to enter at the present time, although we have no hesitation in stating that the majority of the statements of facts accord perfectly with our own pathological observations; that certain experimental researches undertaken by us to test the accuracy of some of the statements, with which we were not familiar, have borne out these statements in the fullest manner; and that we regard most of the doctrines presented in these lectures as affording the best solutions of the several questions discussed which are possible in the actual state of our knowledge upon these subjects.

It is with these feelings that we especially commend these lectures to the study of the medical profession in America. Criticize them, if you will, in accordance with the facts in your possession; criticize them as to either the statements presented, or the logic of the doctrines deduced; but, at least, read them, and think of them. Like the medical doctrines which have preceded them, many of them will no doubt be modified hereafter by the observation of new facts hitherto unnoticed or overlooked.

But such a possibility, even if it could be speedily realized, would not impair the merits of this work. In a developing science, in which new observations are multiplying every day, and which is still in its infancy, to hope that a *theory may last, is to hope that progress may cease.* It was well said by the acute Raspail, to whom reference was made at the beginning of this article, that the best theory is not that which lasts longest, but that which leads most quickly to its substitution by a better; "that which engenders most quickly, and for it, as for certain insects, to engender is to perish."

Tried by this standard, Virchow's book will not be found wanting; already it is bringing forth fruit. In Germany, it has become the stand-point of a host of investigations, and of the most various, friendly, and adverse criticism; and in France and England, a similar effect is beginning to be observed. Everywhere it is producing warm friends or eager enemies, and the discussions of these cannot remain barren.

In conclusion, we may especially commend the English translation, of which the quotations in this article were made. Comparing it critically with the original, we see at once that it is what might be called a free translation, but the author's meaning appears everywhere faithfully rendered, as, in fact, is insured by the circumstance that the translation has been revised by him. The numerous wood-cuts are fac-similes of the originals, and quite equal to them. J. J. W.

ART. XV.—*Un mot sur la Fièvre jaune de Lisbonne en* 1857. Par M. le Docteur GUYON. Paris, 1858.

Report on the Pathology, Therapeutics, and General Aitiology of the Epidemic of Yellow Fever which prevailed at Lisbon during the latter half of the year 1857. (Parliamentary Report.) By Dr. ROBERT D. LYONS. London, 1859. Folio.

THE history of all extensive and fatal epidemics is a subject of interest to medical inquirers. Among the diseases which have furnished examples of the most devastating visitations, in this and other countries, the yellow fever deservedly occupies a pre-eminent position. In respect, indeed, to the number attacked, and to the mortality occurring among these, it has been justly doubted whether any disease, the black plague of the fifteenth century and the Asiatic cholera in our days perhaps excepted, can be compared with it. The oriental pestilence itself, as remarked by a late writer, though it occasioned, a century or two ago, a frightful loss of life throughout many parts of Europe, and though it has given rise, up to this day, at each epidemic return, to a mortality equally large, in proportion to the number of individuals attacked, did not, at the time of its widest diffusion, or, if it did, has long ceased to, produce as great a loss as has resulted from the disease in question. It is and has ever been more circumscribed in the sphere of its prevalence, and has appeared less frequently, even in its more legitimate localities.

To the physicians of the United States the yellow fever constitutes an object of special interest, inasmuch as in some of the cities and towns of the middle section of this country it has often prevailed, and to this day continues occasionally to prevail, with wide-spread mortality; while in many more parts of our Southern and Southwestern States it almost annually shows itself in its most awful aspects.

Influenced by these considerations, we have long thought that we might, without fear of trespassing on the patience of our readers, offer a few facts gathered from the materials within our reach relative to the epidemic which prevailed at Lisbon during the autumn of 1857. Until very lately these materials were very scanty—much more so, at least, than could be satisfactory to any one desirous to form correct notions relative to the origin, mode of propagation, and nature of the disease; for little is to be gleaned from the French and English medical journals we have perused, and we have not as yet been fortunate enough to procure any publication issued by the physicians or public authorities of the ill-fated city. Meagre as they were, however, they appeared to us, in the absence of more ample and authentic documents, to possess sufficient value, in view of the importance of the subject, to justify their being embodied in the form of a short essay, and laid before our readers. To our great satisfaction, Dr. Guyon, in the small volume, and Dr. Lyons, in the very able report, the titles of which we have placed at the head of this article, have in most respects filled up the deficiency here referred to, and we gladly avail ourselves of their labours to enlarge and complete what we had originally intended to say on the subject.

The first of these writers, who holds a high position in the medical staff of the French army, resided a long while in Martinique, where he acquired considerable experience in the yellow fever. The series of experiments he instituted to decide the question of the contagiousness or non-contagious-

ness of that disease, as recorded by Dr. Lefort, is well known. In 1826 he published an essay, in answer to one issued by the last-named writer, on the employment of bleeding and bark in the treatment of the fever; and a few years later he produced another, on the medical topography of Gibraltar, and the origin of the epidemic of that city in 1828.

Dr. Lyons was appointed pathologist-in-chief of the British army in the Crimea, and has given, in his able report on the diseases which prevailed among the troops, the most satisfactory evidence of his qualifications to investigate all matters connected with epidemic occurrences such as those he encountered in Lisbon. Like Dr. Guyon, Dr. Lyons undertook a mission to that city to examine into the pathology and origin of the fever prevailing there, and embodied the result of his observations in the report before us, which is addressed, under date of June 1, 1858, to the President of the General Board of Health of London, and published by order of Parliament.

The city of Lisbon is built in the form of an amphitheatre along the margin of the river Tagus. It occupies an irregular group of hills, which extend to the north for a distance of from two to six miles. These hills attain, throughout various parts of the city and suburbs, a height of 330 to 400 feet above the level of the sea.

"On their inland aspect they fall with a more or less rapid inclination towards a depression which forms in part the valley of Alcantara, and which surrounds the city in the greater part of its extent. Beyond this depression the land rises with different elevations, and extending in the directions of northeast and northwest, is subdivided by the valley which passes from Carnide to Loures into two portions of unequal form and extent. One of these divisions lies to the east and northeast, and the other to the west and northwest of the city of Lisbon, being united near Carnide by a neck of high land upon which are divided the waters that flow to the streams of Alcantara and Odivellas respectively."—*Lyons*, p. 64.

Dr. Lyons states that the main hills upon which the city is built may, he believes, be regarded as not exceeding seven in number, though as many as ten or eleven have been enumerated; and if minor subdivisions of the greater and more salient elevations were to be counted, the number might be still further increased. They have a pretty uniform direction from south to north, occasionally deviating to northwest or north-northwest. They all rise with a more or less rapid gradient from a narrow strip of level bordering the Tagus with a serpentine curve, and probably gained from the river bed at no very remote epoch. The hills and intervening valleys have generally, from the above-named disposition, an exposure to the south, south-southeast, and, in some instances, east.

Dr. Lyons enumerates in detail, under the following heads, the causes which appear to operate most prejudicially against the sanitary state of Lisbon: 1. Defective water supply; 2. Defective sewage; 3. State of shore of Tagus; 4. State of uncleanness of streets in certain quarters; 5. Defective construction of houses, as regards sewage, ventilation, &c.; 6. Existence of general slaughter-houses, tanneries, &c.

1. A large part of the city, as we are told, depends for water supply on the aqueduct of Don John V., a very noble work, completed in the year 1738, which withstood the earthquake of 1755, and is well preserved and kept with great attention to cleanliness. The water it delivers is of fair quality; no record of its temperature exists, and Dr. Lyons is not aware of its having been submitted to accurate chemical analysis. Through about a mile and a half of the extent of the aqueduct, which he examined, the water is perfectly free from organic particles, and not the smallest trace of vegetation is to be found in either of the stone channels through which it

flows. It precipitates in these channels an abundant deposit, which assumes a very clear structure in some places, with brilliant crystalline fracture. The water of this aqueduct is conveyed to numerous public fountains throughout various parts of the city, where it is allowed to run day and night; other fountains exist in two or three parts of the city, which derive their supply from the contiguous hills. In the flat of the city numerous wells exist, which give a supply of less pure water, chiefly used for domestic cleansing purposes.

From data furnished to Dr. Lyons, he is of opinion that the water delivered by the aqueduct may be estimated to be under 500,000 gallons per diem. Of the supplies obtained from the fountains fed by independent springs, from wells, and all other sources throughout the city, it is exceedingly difficult to obtain any accurate estimate. But supposing the water supply from all other sources to equal that from the aqueduct, we shall have 1,000,000 gallons of water per diem to meet all the wants personal, domestic, and public, of certainly little less than 250,000 inhabitants.

"The water supply here allowed is, I am persuaded, far above that actually available, yet for the population assumed, 250,000, and this number is probably below the mark, the allowance of water per head per diem is but four gallons." (The usual calculation allows twenty gallons per head per diem in cities but moderately well supplied with water.) "If, allowing for but a very small annual increase, we estimate the population of Lisbon at 300,000, and calculate the required water supply at the moderate average of twenty gallons per head per diem, we shall have 6,000,000 gallons per diem as the quantity necessary for daily consumption for all purposes, personal, domestic and public. This, compared with the actual supply from the aqueduct and all fountains and wells, taken at a maximum of 1,000,000 gallons per diem, shows that the daily supply of water to Lisbon is equal to but one-sixth of the quantity which would be required to place the population in conditions of salubrity as far as this important hygienic element is considered." (67.)

2. The sewers with which Lisbon is supplied may be considered under four heads: *a.* A system of great sewers. They are five in number, and were intended to drain certain quarters of the city. They open upon the shore of the Tagus. *b.* Besides these, three sewers of minor dimensions, not uniform in size or direction, draining smaller areas, and likewise opening on the Tagus. *c.* Lateral branching sewers, serving limited areas, and having communications with one or other of the systems of discharge into the Tagus first specified. *d.* House drains.

The following conclusions are stated as the result of the examinations of the sewers of the three first classes, as executed by the commission appointed for that purpose, as also by Dr. Lyons. The sewers of the third class were in numerous instances found choked with a foul black semi-fluid, and very noisome matter, giving out very fetid gases, which escaped through the ill-arranged wooden valves which these sewers present at several parts of their course. In certain states of the wind the gases were driven back through the sewers and through the streets to great distances. These gases likewise in some instances escaped through apertures in the foot-paths, and other parts of the streets, diffusing a very foul and pestiferous smell, perceptible at considerable distances.

In many instances, the embouchures of the second and even of the first class of sewers are now below the natural level of the part of the shore of the Tagus at which they disembogue, and in some cases the tide has carried quantities of mud and sand a considerable distance into the sewers.

In the great majority of instances, the embouchures of the sewers, when examined at low water, are found to discharge either no fluid at all, or but

an extremely small quantity of it. In some cases, the fluid so discharged had the appearance of pure water, was not discoloured, and was not in any case more than one or two inches in depth, and did not occupy more than half the bottom of the sewer as it flowed out. Dr. Lyons states that in but one instance was a sewer of large dimensions found to sensibly colour the tidal water at its embouchure, when examined during the rise of the tide and at and after high water. At no time did he observe any fluid, except a small quantity of blackish fluid, to flow from the great sewer which leads from the abattoir or slaughter-house and discharges near the custom-house.

"It is estimated that this sewer receives from the Abattoir above 220 gallons nearly of blood per diem, and at least three cwt. nearly, of boyauderie, animal offal, &c. It also receives, or ought to receive, the dejections and refuse of a quarter of the city, comprising at least 60,000 inhabitants—in all, probably not less than 50 tons per diem! What becomes of this enormous quantity of offal and dejecta? Not one-tenth part of it, by the most liberal calculation, finds its way to the embouchure of this sewer."

Deposits of foul black mud, emitting noisome gases, exist at points of some of the great sewers at various distances from their embouchures. A deposit of putrefying fish was found in one of the great sewers, by the "sewer-rat," the foul emanations of which struck his companion down senseless. The sewers of the third class in all probability form the permanent receptacles of the greater part of the dejections, refuse and offal of a population of not less than 250,000.

"Much of the fluid contents of this system of sewers is probably in a state of constant percolation into and through the adjacent soil, while the gaseous elements escape through the ill-arranged wooden valves and through numerous other apertures, and become diffused through the air."

Dr. Lyons states that the entire system of house drainage is defective, whether reference is made to the dwellings of the rich or of the poor. The water-closet system is all but unknown. It is, however, to the dwellings of the poorer classes that the present observations are meant to apply. In several houses which he examined, in quarters the most opposite to each other, throughout the city generally, the following system of house-drain was the sole and exclusive means Dr. Lyons found for removing the dejections and offals of the inhabitants.

"In the main front wall of the house a vertical shaft was carried, in the substance of the wall, from the highest story to the ground. In communication with this tube or shaft, which was usually rectangular and about six inches in mean section, a number of apertures were constructed on the internal face of the wall, which by a short oblique tube led into the main vertical shaft. One of the apertures, quite devoid of any grating, valve or other protection against the entry of refuse of any kind, however large or solid, was to be found on each landing of the staircase which corresponded to a floor. Into this aperture were thrown by the inhabitants of each floor the dejections, fluid or solid, and the refuse and offal of all kinds accumulating daily in a family.

"In some instances the vertical shaft communicated below with a drain or small sewer leading into the nearest street sewer. It constantly happened, however, that there was no such small drain or sewer, and even where present the drain had, in the majority of instances, become choked and obstructed. Either from the absence of a drain and from the drain being choked, as just described, it was a common condition of things to find accumulations of human dejections and refuse of the foulest kind, in greater or less abundance, poured into the open street; a greater or less heap of such filth, corresponding to the external aperture of the vertical shaft, on a level with the ground, and still more of it carried

by a partial flush of rain or other water towards a shallow, gutter-like depression existing in the centre of some streets.

"Supposing this state of things repeated for a number of houses on both sides of a street in thickly inhabited quarters, and with the super-addition of the effects of the *agoa vay* system, by which still further quantities of ordure thrown from the windows become accumulated in the street, and still assuming a continuance of dry and hot weather for a period of weeks together, narrow streets thus become converted into a kind of open gutter, with the decomposing ordure of a dense population exposed to the rays of the sun, and no attempt whatever made to remove such abominations, and some faint idea may be formed of what is a permanent condition of many crowded streets in some parts of Lisbon."

Well may Dr. Lyons conclude, from what precedes, that the whole system of sewage is defective and ineffectual, and that by far the greater part of the solid and liquid dejections, refuse, and offal, of a population of not less than 250,000 inhabitants, is thus allowed to remain in and about their dwellings, vitiating and infecting the atmosphere they breathe. This accumulation of foul abominations, he remarks, is perpetually increasing, and becoming, as it were, year by year, interstratified with the soil of the city. Gaseous exhalations from the whole system of sewers, but more especially those of the third order, are being perpetually given off, and mingle with the atmosphere. (70.)

3. The Tagus forms a bay, or lake-like expanse, some eight or ten miles in its longest diameter, opposite to Lisbon. Its waters are highly charged with a yellowish-brown silt. The stream is but moderately rapid—five to six miles per hour. The spring tides rise to a height of twelve or thirteen feet; the neap tides rise ten or eleven feet. The ebb leaves exposed to low water a considerable extent of shore in the neighbourhood of Lisbon, very foul, muddy, and exhaling mephitic gases, in various parts. From Belem to Apolonia, the extreme limits of the city, from west and east respectively, the distance is nearly five miles. Dr. Lyons says there can be but one opinion as to the neglected state in which the margin of the city is allowed to remain, and the highly prejudicial influence upon the public health exercised by its existing condition. The portion facing the most densely inhabited parts of the city is only partially supplied with well-built quays; in a large extent it is not so protected.

"Gas-works, cotton, and other factories, several markets and waste spaces, serving for the deposit of the city refuse, collected for sale and deportation by water, are placed immediately upon the shore, and here discharge their aggregated debris directly into the Tagus. Innumerable small streams work their way from the adjacent hills, through an open soil, to the river border; there is also, during low water, a continuous outflow of return sea-water, while the various sewers of the 1st and 2d classes, already described, open upon the same line. These combined causes have produced upon a large extent of this shore a deposit of soft, slimy, foul, and blackish mud, in part fecal, in great part composed of decaying vegetable matter, including minute wood fragments, in large abundance; to these elements are to be added—and it constitutes no inconsiderable item—quantities of decaying fish and fish offal. Heaps of fish entrails, and great numbers of the larger and smaller fish, for which there is no demand for sale or for consumption by the fishing population themselves, are daily cast upon the shore; these masses lie for hours, sweltering in the sun, and are but partially removed by each successive tide." (71.)

In one part of the shore the area left uncovered at low water is estimated at about 400 feet from shore to low water mark. The mud is stated to be extremely fetid, and constantly giving off sulphuretted hydrogen and other mephitic gases. It is a blackish, semi-fluid mud, highly charged with

decomposing animal and vegetable matters. Its depth is variously estimated at from about one foot to four and five yards.

The extent and rapidity of the river deposit is such, that it is found necessary to keep a dredging machine constantly in operation, to maintain the basin in the above condition of even moderate freedom from deposit. Without this, it would become in a short time completely filled up.

"The conditions of the shore of the Tagus here described constitute, undoubtedly," as Dr. Lyons adds very justly, "a source of permanent insalubrity, and must be highly prejudicial to the health of the population of the portions of the city in the vicinity of the river. The exhalations given off from the area of foul, putrescent mud, which borders a considerable extent of the city, and which lies exposed for hours to the rays of the sun, are carried by the wind, whenever it has any southing, into the upper parts of the town. No morass can be supposed, which, for purposes of insalubrity, could be made to occupy a more favourable position with regard to any city, for the effectual development and diffusion through a great mass of the population, of its pestiferous emanations, than this foul mud area which borders a large extent of the river margin of Lisbon."

4. It is stated that throughout a large portion of its superficies, Lisbon presents the appearance of a well-paved, well-flagged, and well-macadamized city; that the permanent way is well paved and well kept; and that the condition of the streets, public places, and even of the lanes and alleys in remote and backward quarters, is, in many respects, highly creditable to the authorities. But, it is added, the absence or obstructed state of sewers, the want of privies and of proper house drains, and the system of vertical shafts, before described, all combined with the incorrigible habits of the population, which seems to have never known, or to have completely forgotten, the privacy, delicacy, and retirement, almost universally observed elsewhere, in satisfying the calls of nature, have led to the conversion of the streets, lanes, and alleys, in whole quarters of the city, into the common receptacles for the human dejections, vegetable and animal garbage, and offal of all kinds, of large masses of the population.

"These foul abominations lie exposed to the air and sun till decomposed and trodden into the soil, or partially flushed away by heavy rain falls. Many of the streets of the quarters alluded to are thus converted into permanent lakes. It may be remarked here that the Koprological studies thus forced on the eye of the observer, in so many and such opposite quarters of the city, lead to the conclusion that a confined habit of body has become a constitutional state with a large proportion of the population, both male and female, young and old. Medical testimony unanimously bears out the observation." "The *agoa vay* (water goes) system, so-called from the caution to passers by, shouted from the windows in the nightly process of throwing into the streets the contents of certain domestic utensils, is now confined to the back streets and quarters."

5. Dr. Lyons is justly of opinion that the defects of house construction at Lisbon are important causes of insalubrity, and that the chief of these defects which bear upon our present purpose may be briefly classed as follows: A. The total absence or small and confined state of rear premises in all houses. B. The total absence or imperfect state of privies, sinks, and house drains. C. The want of thorough ventilation and front and rear aeration and lighting. This is especially obvious when, as in the great rectangular blocks of houses which form the superb-looking main streets of the flat of the city, the houses in parallel lines are applied nearly back to back, so that a very narrow interval between the rear of these very lofty five to six storied houses exists, totally inadequate for the purposes of ventilation. The main staircase of many of the finest and most massively built houses

in this and other quarters is pitch-dark at noonday at the first and second landings. A close, confined, and in some instances very foul smell prevails in many of these houses. These conditions are to be understood as occurring in the better class houses as well as in those of the poorer classes; they are, however, still further exaggerated in the case of the latter.

Dr. Lyons, from whom the foregoing statements are borrowed, remarks that Lisbon may be said to be throughout, even in the remoter quarters inhabited by the lower classes, a substantially and even massively built city. If the vices of construction above described were removed, and a good system of sewage and house drainage adopted, it would not be easy to suppose a population of equal extent so well housed, as far as regards the physical elements of their dwellings.

6. The last important cause of insalubrity mentioned by Dr. Lyons is to be found in the existence within the city of the public slaughter-house, tanneries, factories of various kinds, &c. &c. In the general slaughter-house or abattoir—

"The number of animals dispatched annually amounts to from 25,000 to 30,000 head of cattle. This number does not include the pigs annually consumed in Lisbon, which are killed at various places throughout the city and on the opposite shore of the Tagus. The following passage from a report of one of the commissions shows the state of the abattoir in a striking manner: 'The exhalations of the soil (saturated with animal products) were so fetid that they could not be supported by the veterinary officer in charge. I,' says the member of the commission especially charged with the inquiry, 'no sooner received the first breath of them, than I was attacked with vomiting and headache. The vomiting ceased when I withdrew to a distance, but the headache continued until dinner-time.'"

To all this the author adds, that although many other causes of insalubrity could doubtless be enumerated, the foregoing are, in his opinion, the most effectively operative. It is, however, to be observed, that some causes of insalubrity met with so constantly in other cities are wanting in Lisbon; as, for instance, intra-mural interments. Three well-organized public cemeteries exist on the outskirts of the city.

Dr. Lyons enters fully upon the general and special climatology of Lisbon. On this subject we cannot, and indeed need not, offer more than a few remarks. He states at the outset that, according to Colonel Franzini's excellent meteorological observations made since 1816, it would appear that at Lisbon the year should be divided as follows: The months of December, January, February, and March form the winter; the spring only lasts during the months of April and May; the true summer continues during June, July, August, and September; and autumn only includes the months of October and November.

The following general conclusions as to mean results, drawn from the observations of sixteen years of the meteorology of Lisbon between the years 1816 and 1840, are recorded by the above cited accomplished academician:—

Barometric Pressure of the Atmosphere.—The mean height of the barometer is stated to be 30.24 inches. In general, the barometer reaches its greatest elevation in the clear days of the winter season, when the prevailing winds are N. and N. E.; its least elevation is during the prevalence of the S. and S. W. winds.

Temperature.—The mean temperature deduced from Col. Franzini's observations respecting the climate of Lisbon is 61° Fahrenheit. This, with slight variations, is the temperature of the month of October. Range from 26° to 105.°

Mean Winter Temperature.—The mean winter temperature, as deduced from the same observations, is 52°.1 (26°—85°).

Mean Spring Temperature.—The mean spring temperature is 60°.5 (37°—89°).

Mean Summer Temperature.—The mean summer temperature is 70°.4 (48°—105°).

Mean Autumn Temperature.—The mean autumn temperature is 59°.5 (34°—83°).

Rain-fall.—The mean annual rain-fall, as given in Franzini's table, is stated at $24\frac{1}{2}$ (nearly) inches. It is elsewhere given as 23 (nearly) inches, distributed over 98 rainy days. Two remarkable extremes were observed—$10\frac{1}{3}$ inches in the driest year, and $36\frac{3}{4}$ inches in the most rainy year.

Rainy Days.—The rainy days, 98 in number, are found to be very unequally distributed. In general, the two months of autumn, October and November, and the first two months of winter, December and January, were the most rainy. June, July, August, and half of September were usually pretty dry.

Winds.—The N. wind prevailed on 144 days; the S. W. on 138 days; the N. W. on 124 days; and all the rest in due proportion. There were 129 days of wind more than usually strong, and 12 days of actual storm.

The foregoing observations may be summed up as follows:—

Barometer (mean) 30.24 inches.

Thermometer (mean) 61° Fahr. Winter (mean) 52°.1; range 26°—85°. Spring (mean) 60°.5; range 37°—89°. Summer (mean) 70°.4; range 48°—105°. Autumn (mean) 59°.5; range 34°—83°.

Rain-fall (mean) 23 inches (English); range $10\frac{1}{3}$—$36\frac{3}{4}$ inches.

Rainy days (mean) 98.

Prevailing winds N. N. W. to S. W.

Cold days (mean) 57.

Hot days (mean) 58.

Windy days (mean) 83.

When we compare these results and those obtained in 1855 and 1856 with corresponding ones noticed in 1857, we find slight modifications in the latter year, not unlike those that have occasionally occurred elsewhere during epidemic seasons. The barometrical maxima of 1857 are below those of 1855 in every month except October, November, and December. The minima of 1857 are likewise below those of 1855, except in February, March, May, and December.

The maxima of 1857 are likewise less than those of 1856, except in January, April, June, and July; while, except in May, June, August, October, and December, the minima of 1857 are greater than those of 1856.

In regard to the monthly extremes, we find that the highest maxima were in December, 1856; December, 1857; January, 1855; May, 1855; January, 1857. The lowest minima were in January, 1856; in November, 1857; March, 1856; and February, 1856. In the table of barometric means the highest will be found to occur in December, 1857; June, 1855; January, 1855; July, 1855; and December, 1856. The lowest means were in January, 1856; November, 1857; May, 1857; August, 1857; and October, 1857. The means for March, May, June and August, 1857, are lower than the corresponding ones for both 1855 and 1856. The means of 1857 are greater than those of 1855 only in the months of February, October, November and December, and less in other months; they are greater than those of 1856, in January, April, July, September and December, less in the other months.

The greatest monthly variations were in January, 1856; February, 1855; November, 1857; December, 1856; March, 1856. The least

variations were in July, 1857; July, 1856; August, 1855; July, 1855; and September, 1855. The monthly variations in 1857 were less than those of 1855 and 1856, in February, March, April, July, September, October, and December.

In respect to the thermometric data of 1857, we find from a summary furnished by Dr. Lyons, that in February, March, April, May, July, and October, the maxima of 1857 exceed those of 1855 and 1856. In every instance except one, August, the maxima of 1857 are higher than those of 1855. From February to July, 1857, a steady increase in the maxima is observed; and with the exception of the maximum for June, 1857, which is below that of June, 1856, these maxima are in excess of those of the two antecedent years. It is to be remarked, however, that the maxima of 1857 are, with the exception of those of June, September, November and December, *below* those of 1855 and 1856. The highest maximum for the three years 1855, 1856, 1857, is found to have occurred in July, 1857, when the thermometer marked 99°.50 F.

The variations for February, March, April, May, July, October, and November, 1857, were remarkable; and with the exception of the latter month, in which the variations were considerably less than in 1857, the monthly range of the thermometer was much greater in 1857 than in 1855 and 1856.

In comparing a summary given of the monthly means of 1855, 1856, and 1857, we find an excess in those of April, July, September, October, and November, 1857, over the corresponding ones of 1855 and 1856. This excess, however, is very slight, except in the months of July and November, in which it is 1°.61 and 1°.91 respectively. The means of January, February, March, May, June, August, and December, 1857, are below those of the corresponding months of 1856; while in February, August, and December, 1857, they are below those of 1855. The lowest mean is that of January, 1855; the highest that of July, 1857. The means of the aggregate means for each of the three years 1855, 1856, and 1857, were 58°.362 F.; 60°.792 F.; and 60°.180 F., respectively.

It may be here remarked that the means of 1851 are said to have fallen below those of an antecedent period of 16 years, recorded by Franzini, in all the months except July, September, and November. Dr. Lyons, however, entertains great doubts as to the propriety of comparing the means of 1857, as deduced from the records of the royal observatory, with those of Franzini's 16 years period of observation. His reasons are founded on the higher position of the locality where the former were taken, its exposure to the northwest and east winds, its distance from the Tagus, and other influential circumstances, which have probably the effect of keeping the extreme and mean temperature there below the degree it attains in the proximity of the river, where the denseness of the population, the houses, fires, and above all the reflected heat of the sun thrown back from the amphitheatrically arranged group of hills which border that stream, must unquestionably raise the mean heat of both night and day. The objection would seem to be sustained by the fact that a table exhibiting the mean and mean maxima and mean minima for nine years, deduced from observation conducted by Dr. Martin, in a locality contiguous to the Tagus, to the centres of business and population, and to the main force of the epidemic of 1857—a locality similar to that in which Franzini is supposed by Dr. Lyons to have made his observations—shows that in every instance except one, July, the means of 1857, as recorded at the observatory, are below those of the nine years period in question. They exceed those of Franzini.

In conclusion, Dr. Lyons states that he regards Dr. Martin's observations as entirely reliable, and that they show an excess of temperature in the epidemic months of 1857.

The total fall of rain was less in 1857 than in 1856, and far less than in 1855. The quantity in the last mentioned year was 41.074 inches, the average quantity annually, being 23. In 1856 it was 34.706 inches, and in 1857 only 32.048. In 1855, during the seven last months of the year, the quantity that fell amounted to 497.8 millimetres, about 19 inches. In 1856, it did not exceed, during the same period, 188.1, $7\frac{1}{4}$ inches; while in 1857, during the epidemic months, it rose to 446.0, $17\frac{1}{8}$ inches. No great difference, therefore, presents itself, in that respect, between the yellow fever season of 1857, and the non-yellow fever of 1855; while in 1856, when the disease existed to a limited extent, the quantity of rain that fell was comparatively small. In 1857, the fall prior to June amounted to 367.8 millimetres. In 1856 to 690.6, and in 1855 to 545.5. From this comparison, we may perceive that, so far as the quantity of rain that fell is concerned, nothing can be made out respecting the cause of the severe epidemic of 1857, and of the lesser one of 1856. The same remark is applicable to the degree of the dew-point, elastic force of vapour, and weight of vapour, observed in the months of July, August, September, October, November, and December, of the above mentioned three years; for, on comparing the means of 1854, 1855, and 1856, with those for 1857, we shall obtain the following results for the latter year:—

"*July.*—There was an elevation of the dew-point in July, when it reached a maximum, 60.25. The elastic force of vapour was likewise at a maximum in the same month. The weight of vapour to the cubic foot of air was below the maximum in this month, as 5.83 (1857) is to 6.005 (1855).

"*August.*—The dew-point in this month was above the minimum (59), but was below the maximum, as 59.2 (1857) is to 60.75 (1854 and 1855). The elastic force of vapour was considerably above the minimum, .373 (1856), but below the recorded results .533 (1854), and .568 (1855), respectively. The weight of vapour to the cubic foot of air was above the minimum, 5.63 (1856), but below the recorded results 6.05 (1854), and 6.075 (1855).

"*September.*—The dew-point was higher than in the two preceding years, having reached 60.5 (but not so high as in 1854, when it was 60.75). The elastic force of vapour, .534 was above a minimum, .328 (1856), but below the maximum, .557 (1854). The weight of vapour to the cubic foot of air was below the maximum, 6.08 (1854), but above that of the other years.

"*October.*—The dew-point was at a minimum, 54.1. The elastic force of vapour .457, was above the minimum, .306 (1856), but below the results of the other years. The weight of vapour to the cubic foot of air was at a minimum, .508.

"*November.*—The dew-point was at a very high maximum, 54.5. The elastic force of vapour was likewise at a maximum, .456. The weight of vapour to the cubic foot of air was also in excess.

"*December.*—The dew-point 50, elastic force of vapour .411, and the weight of vapour to the cubic foot of air, 4.62, were all in excess over the similar elements of the corresponding months in the previous years."

Dr. Lyons observes that, in 1857, the dew-point fell but 10.25 degrees from July to December, and that the minimum depression for the same period, in the three preceding years was 12.85.

From all that precedes, he properly concludes that the Lisbon epidemic of 1857 had no such necessary connection with a high dew-point, as would be made probable from the results obtained by inquiries into the history of other epidemics.

We may mention that the epidemic of 1857 was not the first from which

the city of Lisbon suffered. We have satisfactory proofs that the disease appeared there and prevailed extensively, in the autumn of 1723, during the reign of King John the 5th. The epidemic is described in a Special Essay, by an eye-witness, Dr. S. F. Da Cunha,[1] and is mentioned by several other native writers—José Rodriguez de Avreu, physician of the king; Manuel de Silva Leitão;[2] Joa Mendes Sachetti, and Duarto Rebello Saldanha. Both de Avreu and Leitão were, like Da Cunha, contemporaries of the epidemic. The latter, after having prevailed during three months, ended on or about the 15th of October, and caused a mortality of six thousand. By Leitão and de Avreu it was designated by the name of *vomito preto;* while Da Cunha applies that denomination to one of the worst and most characteristic symptoms of the fever—the black vomit. The disease is also mentioned, in some detail, by Dr. Gilbert Kennedy, physician to the British Factory at Lisbon, in a letter addressed to Mr. Cayley (H. B. M. Consul), in answer to a communication requesting information on the subject.[3] The fever is also alluded to in the Eulogium by Vicq-D'Azyr, of Dr. Rebeiro Sanchez, another eye-witness of its ravages.[4]

Of the yellow fever nature of the disease they encountered, there cannot be the least possibility of doubt. Independently of the fact that most of the authorities cited referred to the black vomit as one of its characteristic symptoms, the description of the complaint left by Dr. Kennedy, and his statement of some of the circumstances attending its development and progress, cannot permit us to admit the possibility of these being applicable to any other form of febrile disease. Let the reader judge for himself. Dr. K. remarks:—

"The heat last summer began late, but continued very violent, and much longer than usual, so that the grapes, which were more plentiful this year than many years past, were burnt up, when pretty ripe. All the summer was sickly, but about the middle of August there appeared the fever which now reigns, accompanied with a pain of the head and loins, a great sickness at the heart and stomach, with retchings to vomit, very contagious in the lower parts of the city, going generally through a family, and very few families escaping, especially in the close, narrow streets. The high parts are much freer than the lower parts, and the villages and country houses about town are entirely free from this distemper, notwithstanding the great communication. The recovery is generally accompanied with the yellow jaundice. The only mortal symptom in this epidemic is the vomiting of black choler."

The stomach, on dissection, was found to contain a large quantity of this fluid. Leitão states, as a fact well known, that the number of persons who died that year, from the 15th of September to the 15th of December, was over 6000, "most deaths where most dirt," where vapours arose by reason of heat, and the winds which prevailed were from the west; those who were most free were those who lived on the higher parts, and in the larger houses. (p. 43.) Of the same epidemic Da Cunha remarks that the summer of that year was very hot and dry, the autumn partaking of the same character, while the antecedent years likewise had been dry. As the result of this, the city of Lisbon became very sickly, in both its occidental and oriental divisions.

[1] Discurso e Observaçôes Appolineas, Sobre as doenças que houve na cidade de Lisboa Occidental e Oriental O Outono de 1723. Lisboa, 1726.

[2] Arte com vida ou vida Comarte, per le Dr. Manuel de Silva Leitão. Lisboa, 1738.

[3] Lyons, p. 110. See also Bancroft on the Yellow Fever, 436. The letter is preserved in the British Museum.

[4] Hist. de la Soc. Roy. de Méd., iv. 215; note. See also the collected works of Vicq-D'Azyr, iii. 225; note.

"The diseases were accompanied with continued fever, pains in the head, and laxity of body, some presenting horripilations, others not, most of them having nausea, without vomiting anything. Some presented anxieties, and in others the superior region of the stomach was painful, so that they would not allow it to be touched. The disease was very acute. Most of the patients vomited black matter, and had dejections of the same kind, and they died on the third, fourth, and fifth days."

It would appear from the statements of de Avreu, that the yellow fever prevailed in 1721, in Ericeira, a village situated at a distance of seven leagues from Lisbon; and in 1728, at Penihe, another village one or two leagues nearer that city. The disease again prevailed, though less extensively, at Lisbon, in the months of August and September, 1724; and finally, from de Avreu we learn that it broke out, and assumed the epidemical form at Funchal, in the island of Madeira, in the year 1738. Some modern writers speak of an invasion and wide diffusion of the disease in Lisbon in 1736; but there is reason to believe, as Dr. Guyon—who has investigated the question—informs us, that the epidemic referred to that year is, in reality, the one described by Da Cunha, and others writers cited, as having occurred in 1723.

More certain is it that the fever prevailed in 1856 in the last-mentioned city, as also at Belem a populous town situated a few miles southeast of the former place. The visitation on this occasion was of somewhat over three months' duration—commencing on the 5th of September, and ending on the 28th of December. The disease presented the same characteristic features as it did the year after; but the yellow fever being unknown to the physicians of the place, the true nature of the cases they observed was not satisfactorily made out by them till the succeeding epidemic, when the phenomena of the prevailing complaint were recognized to appertain to the fever in question; and their similarity to those of the preceding season served to establish the true nature of these. To the end of October there were 100 cases at Belem, and 30 deaths; till the same date there were 63 cases and 16 deaths, and in Rua de la Bica (in the same period) 62 cases and 4 deaths.

In the month of November there were 86 cases and 21 deaths in the Rua St. Roque and Rua de la Bica; a hemorrhagic tendency was generally observed throughout the city. The disease commenced at Belem, in the court of the old custom-house, "and afterwards passed into the Rua de Embaixador. It is stated that the first case of yellow fever which occurred in St. Roque was in the person of a chocolatier (chocolate-maker), who died September 23d." The patient is said to have had no intercourse with persons coming from Oporto (in which place the fever raged), nor with the lazaretto, nor with the custom-house. On the day he sickened he had walked through the market. This patient resided in the Rua St. Roque, No. 60. The fever subsequently appeared in the house No. 57 of the same street, in which it attacked 16 persons.

The number of cases reported at Belem, St. Roque, and Bica, in 1856, amounted to 311, with 71 deaths, or 1 in 4.380.

A case of yellow fever, in a soldier, is reported as having occurred at Oporto, in 1856, as early as the 25th of July. From this date to the 10th of September 70 cases occurred in that city, of which 42 died. It was chiefly confined to the quarter Miragaia, remarkable for its filth and insalubrity.

The epidemic of 1857, like that of 1723, made its appearance during the first fortnight of August. Some cases, indeed, were noticed earlier. But,

as usually occurs under circumstances of the kind, the cases were at first few in number, and their nature was not fully ascertained. One case is recorded as having been verified so early as May; another was seen in July; while there appears to have been at least five in August. The existence of the epidemic was not officially announced till the 9th of September, when three persons were admitted into the hospital, and two deaths were reported. No return of cases occurring in domiciles is found till the 16th and 17th of September, on each of which days one case is recorded. There is then an interval of ten days, during which no return is made of cases in domicile. On the 27th of September 13 cases of the kind were reported, against 44 in hospital. On the 9th of October this proportion had become reversed, the number of those reported being—in hospital 106, in domicile 156. From the 9th of September the disease gradually spread, the cases daily increasing in number. Nevertheless, on the 2d of October the number received in the hospitals since the outbreak of the disease did not exceed 712 (609 males and 103 females). Of these 712 cases 194 died (167 males and 27 females), while 321 remained under treatment (266 males and 55 females).

The epidemic attained its culminating point of intensity, both as regards the number of cases and the mortality, during the second half of October—between the 19th and 28th. The largest number of cases reported, and the greatest loss of life, occurred on the 24th, when the former, both in hospital and private houses, reached the number of 298, and the deaths amounted to 173. On the 19th the mortality was 141, and on the 23d 152; while on the 26th it amounted to 122, on the 27th to 152, and on the 28th to 121.

From the period mentioned (24th) the fever lost its force, and closed its epidemic career about the end of December; though straggling cases continued to present themselves at a subsequent period, and some twenty occurred as late as the early days of March. But though the number of cases diminished in the way mentioned, the disease did not lose its malignancy in a corresponding degree; for the mortality in proportion to the attacks remained to the last much the same as it was at the outset of the epidemic. On the 17th of November the whole number of cases reported from the commencement amounted to 10,556. This number, though large, probably falls short of the real one, inasmuch as many cases may, as has occurred everywhere, have failed to be reported, either designedly or through ignorance, on the part of the medical attendants, of the true nature of the disease. The mortality during the same period is stated, with a greater likelihood to correctness, to have reached 3,550, or about 1 in 3. Dr. Lyons states that, according to the official returns, the totals of attacks, cures, and deaths, to the 25th of December, amounted to 13,481—5,652 in hospitals, and 7,830 in domiciles. Of these, 4,753 died, or 1 in 2.836. In September the cases were 662, and the deaths 190, or 1 in 3.484. In October, cases 6,531, deaths 2,077, or 1 in 3.149. In November, cases 5,327, deaths 2,046, or 1 in 2.603. And in December (to 25th), cases 961, deaths 440, or 1 in 2.184. According to M. Guyon, the whole number of cases, from the outset to the close of the epidemic, exclusive of those occurring among the military, amounted to 19,510. Of these, 5,834 were treated in the hospitals, the rest in private houses. Of the hospital cases, 4,718 were males, and 1,116 females. The whole mortality out of the 19,510 cases reached 6,859. That in the hospitals alone amounted to 2,063—1,679 males, and 384 females. The number of cases among the military at the Marianos Hospital, from the 22d of September to the 31st of December, is stated to have been 585, with a loss of 116. To this must be added 37 cases at the

hospital of Belem. Of these, 2 died; making a total of 622 cases and 118 deaths; being in the proportion of 1 death in 5.271 cases. The garrison of Lisbon is about 4,300, and of Belem 500, which gives us 1 case in 7.4, and 1 death in 40.67. Of British and Irish residents—the total number of whom is not known accurately—28 died, according to an official return to the consulate. By a private return made to Dr. Lyons, through the kindness of the heads of the British and Irish firms, he finds that 47 were attacked (including servants), of whom 20 died. Of the extent of the disease and mortality among the sailors no satisfactory information has as yet reached us. All we know is, that in the marine hospital 47 cases were treated from the 8th of September to the 30th of November, inclusive. Of these, 16 died, 27 recovered, and 4 remained under treatment at the date of the report. Independently of the cases and deaths here mentioned, many—not a few not registered, or, if registered, not officially reported—occurred among foreigners. What the exact number was we are not told. All we can gather on the subject is that of the French alone not less than 35 or 40 died; which, at the usual rates of the mortality, would give from 105 to 120 cases.

Now if, assuming as correct the returns of cases and deaths as given by Dr. Guyon, we sum up the whole of these various items, we shall find that the proportion of deaths to cases amounted to 1 in 2.85 The mortality varied at the several periods of the epidemic from 1 in 2 to 1 in 3. In the hospitals it was 1 in 2.88, and in private practice 1 in 2.65. The population of Lisbon being about 250,000, it follows that the mortality amounted to 1 in 36.42. If, however, we take into consideration that the number of persons who emigrated did not fall short of from 30,000 to 50,000, we discover that the loss in reality reached a much higher proportion.

It may not be improper to remark, in connection with the subject of the mortality occasioned by the yellow fever during this truly memorable epidemic, that many of the cases and deaths here ascribed to the yellow fever ought perhaps to be referred to intercurrent complaints. Dr. Guyon, from whose essay the foregoing statements are principally borrowed, would lead us to conclude that the mortality from the prevailing fever was less considerable than we have stated it to have been—the number mentioned having reference to the general loss of life in the city during the progress of the epidemic, and not to that occasioned by that fever alone. In a foot-note at page 20 he informs us that the entire mortality during the whole of the year 1857 was 11,752; and adds, that inasmuch as the mean loss of life during each of the fourteen years anterior to 1856 (1842–1855) was 6,985, it follows that the excess of mortality in 1857—4,767—must be regarded as representing the abnormal loss occasioned by the yellow fever. When, however, we take into consideration that whenever, in this and other countries, the fever assumes an aggravated epidemic form, and spreads widely, it takes the place of most other complaints which otherwise would figure in the mortuary list, or, complicating them, imparts to them a fatal tendency, we may reasonably infer that the same results must have obtained at Lisbon; and that while a certain portion of the deaths included in the amount mentioned above—6,857—may not have been the offspring of the prevailing fever, a large number of those supposed by Dr. Guyon to have been produced by intercurrent complaints were in reality due to the other, directly or indirectly.

The conclusion to which Dr. Lyons has arrived relative to the total mortality are somewhat different from those just mentioned. He states that the total recorded number of deaths from the epidemic amounted to 4,753; but remarks that the numbers here given can be accepted only as

an approximation to the true figures; inasmuch as it is very doubtful whether in any epidemic the method of direct enumeration from hospital and other returns can be supposed capable of giving results free from large error, arising from the difficulty of determining an absolute diagnosis of all cases and the omission of many well-defined instances of the prevalent disease. Deducting from the 13,481 cases reported to the 25th December, 134 as the number of cases wrongfully reported as yellow fever, and adding 1,000 as the probable number of true cases omitted, he arrives at the conclusion that the probable amount of cases reached 14,347. After recalling the fact already mentioned, that the mortality for the year amounted to 11,752, and that the annual mortality being 6,985, the loss of life from the epidemic has been estimated at 4,767—this being the amount above the average—he remarks that this cannot be assumed as the total mortality of the epidemic, and quotes the following conclusion, drawn by Franzini:—

"The average mortality for the four months, September to December inclusive, is only 2,528; the mortality in those months of 1857 amounts to 7,874; the difference is 5,346, which may be fairly ascribed to the epidemic, and which if we include the burials from yellow fever cases in the British, German, and Jewish cemeteries, may be safely stated at 5,500 for the whole population. This may be taken as the safest approximation to the mortality from the epidemic."

Dr. Lyons adds, that a very constant proportion of deaths to attacks, as 1 to 3, is observable at variable periods of the epidemic, from which Franzini deduces that the total of attacked must have reached from 16,000 to 17,000.

But whatever be the truth in this matter, the mortality which occurred on this occasion was certainly a very large one, and cannot fail to be a subject of sad remembrance to the inhabitants of the Portuguese metropolis. Considerable, however, as it must be admitted to be, Lisbon may yet console herself at having suffered less than other cities on this and the other side of the Atlantic. When we inquire, for example, into the sum of the loss of life in proportion to the entire population and to the number of the sick consequent on some of the epidemics of yellow fever which have visited this city, we shall find a confirmation of this statement. The three sickly seasons of 1793, 1797, and 1798, give us an average of one death in 14.24 of the entire population, and of one in 10.03 of those who remained. The mortality among those attacked, estimating it from the results recorded in each epidemic year, from 1793 downwards, varied from one in 1.2 (1819) to one in 3.86 (1805)—giving an average for all those epidemics of one in 2.12. In other parts of this country the extremes of the ratios has ranged from one in 1.5 to one in 13.1. Excluding the latter epidemics as being exceptional, from the unusual mildness of the disease—the average ratio of all the statements, including those relative to the mortality in public institutions, is one in 2.53—the extremes being one in 1.2 and one in 6. In Spain the disease has occasioned a still greater loss of life. The year 1804 one of great calamity there. On that occasion, twenty-five cities or towns were severely visited by the fever. The population in these amounted to 427,228, of which not less than 52,559, or one in 8.12, perished. In fourteen of those places, at different periods, the mortality in proportion to the population was one in 6.42, the extremes being one in 2.25 and one in 13.3. In seven places, the proportion of persons affected was one in 3.087, the extremes being one in 1.3 and one in 6.42; while the hospitals gave a mortality of one in 2.15 of the number admitted, with extremes of one in 1.1 and one in 3.82. The whole number of items noted in the table published by Dr. La Roche, from whose work on the yellow fever the foregoing

statements are borrowed, gives an average of one in 3.55, the extremes being one in 1.1 and one in 9.6.

If we now turn to the results obtained in tropical climates, we shall perceive that, laying aside a few instances when the disease would seem to have assumed an unusually mild character, or to have mostly or exclusively prevailed, as at Boa Vista, among individuals who, from constitutional peculiarities, or the effects of acclimatization, are little liable to the severe forms of fever; or when it has presented itself in a garb calculated to justify doubts as to its genuineness, the mortality there, though sometimes falling short of that experienced at Lisbon, more frequently has exceeded it. Omitting the instances of an exceptional kind referred to, the extremes will be found to range from one in 1.08 to one in 10—the average of all the ratios being one in 2.32. Take, in further illustration, the occurrences at Gorée and St. Louis, Senegal, in 1830. In the former place, 53 Europeans out of the 150 whites constituting the whole of the civil population, died. The cases amounted to no less than 144, being within only six of the entire number of white persons exposed. The deaths, therefore, were in the proportion of one in 2.83 of the population, and one in 2.7 of the sick. In St. Louis, 308 out of 650 died. Scarcely a single individual of the susceptible population escaped an attack. The deaths were in the proportion of one in 2.63 cases, and one in about the same number of inhabitants.

Of the nature of the disease of Lisbon in 1857, little need be said. That it was the true yellow fever there can be no doubt.

Dr. Lyons gives a full, and, as it would seem, a very accurate description of the disease. He remarks that the cases resolved themselves naturally into certain groups, more or less distinguished from each other by the presence or absence of well-marked and characteristic clinical features. This, we scarcely need say, occurs everywhere, and at all epidemic times. Among the groups thus distinguishable from each other, the following were, according to his observations, perhaps those best marked, and most readily recognizable: 1st. The Algid Form. 2d. The Sthenic Form. 3d. The Hemorrhagic Form. 4th. The Purpuric Form. 5th. The Typhous Form.

It will be unnecessary to follow Dr. Lyons in a detailed account of the disease, under these several heads, which embrace all the cases of it which present themselves, in greater or less number, in times of wide extended epidemics. We have only room for a few general remarks. Dr. Lyons states that, regarding the epidemic in question in a general way, with a view to the determination of its leading nosological features, it presented all the characters of a well-marked fever. The nervous system was oppressed, the circulation highly excited, while the secretions were diminished in quantity, altered, or suppressed. The temperature was elevated, the skin hot and dry to the touch, and the thermometer in the axillæ indicated an increase above the natural standard, frequently to the extent of two, four, and sometimes six degrees Fahrenheit.

The sensorial functions were but comparatively little affected on the whole, and it was common for patients to retain possession of their faculties till shortly before death. Exceptions, however, occurred to this rule, and cerebral excitement, with delirium, and other head symptoms, were observed in a certain number of cases. It was difficult, Dr. L. continues, to reduce the disease to any distinct type of febrile action; strictly speaking, it did not correspond to any of the regular types of continued, intermittent, or remittent fever; the phenomena were, undoubtedly, perfectly continuous in numerous cases, for days together. Intervals also occurred in which the

patient was devoid of pyrexial excitement of any kind, but these intervals were irregular in their periods of occurrence, and not by any means constant, and they were certainly wanting in any definite character of periodicity. In his judgment, the disease is best described as a fever composed of distinct stages or periods, the characters and the order of occurrence of which, though by no means constant and uniform, observed much regularity in the majority of cases.

At Lisbon, the most commonly prevalent form was the algid. Next came, in point of frequency, the hemorrhagic; next again, the sthenic, while the purpuric occurred in a limited number of cases, and the typhous only occasionally. The algid form was, as its equivalent is elsewhere, generally speaking, that which presented the most rapid course, the earliest and greatest amount of prostration of the vital powers, and which, likewise, offered in most frequent combination the most appalling characters of the epidemic re-united in individual cases. Dr. Lyons well observes there can be no doubt—

"That a great deal of the discrepancy of statement and conflict of opinion in the accounts we possess of the several epidemics of yellow fever, occurring in various localities, has arisen from the different forms which the disease presents being confounded in one common description, embracing the phenomena of all. It will, I think, be pretty evident that the description of an epidemic, on which the form I shall describe as the algid form predominated, must differ essentially from that of an epidemic in which the sthenic, the hemorrhagic, or the purpuric varieties were most generally presented. The same holds with respect to the typhoid form, which, though not a prevailing type in the Lisbon epidemic, must, when presented largely in any visitation of yellow fever, give a special character to it."

Dr. Guyon, though not offering an elaborate description of the disease, calls attention to some points relative to which it varied more or less from the cases noticed somewhat frequently in this and other countries, not only during different periods of the same epidemic, but during different sickly seasons. Generally speaking, there was an absence, or less intenseness, during the first stage, of headache, and pain in the loins and limbs. The febrile symptoms were moderate. Indeed, many of the patients felt so slightly indisposed, as to be able to walk without assistance to an hospital. But whatever may have been the aspect of the disease, during the first stage, its characteristic features did not fail to manifest themselves at a subsequent period; while the diagnosis was confirmed by the post-mortem examination of those who perished. In relation to this point Dr. Lyons makes the following statement:—

"I am not aware of the occurrence during any portion of the Lisbon epidemic of 1857, of that class of rapidly fatal cases, vernacularly known elsewhere as 'walking cases,' and though rachialgia was a common symptom, I did not observe nor hear of its occurrence in that intense form accompanied with sudden prostration of the vital powers, and rapidly fatal issue, known so constantly in the older epidemics, as 'coup de barre,' or stroke of the bar, from the suddenness and violence of the attack of pain in the back or loins, and the 'knock-down' influence of the disease upon the patients from the first moment of invasion."

We must not neglect to mention that similarly to what has not unfrequently occurred elsewhere, and perhaps more particularly during the last few years, the black vomit at Lisbon did not prove as invariably fatal in 1857, as it appears to have done in 1723, and, indeed, as it has been wont to do in many places, and during the greater number of epidemics. So far from this being the case, a number of recoveries occurred after a free

discharge of this dreaded matter from the stomach of both male and female patients.

Nothing satisfactory relative to the pathological anatomy of the disease can be gathered from the work of Dr. Guyon. We learn from it, however, that this important subject is far from having been neglected by the Lisbon physicians, and we may expect to receive, before long, the results of their researches. In the meanwhile, we may state as a fact worthy of the special notice of all American physicians, and for a knowledge of which we are indebted to a recent French writer,[1] that the dissections performed at Lisbon, revealed the existence of the fatty degeneration of the liver, a pathological condition first pointed out, so far as the yellow fever is concerned, by our distinguished countryman, Professor Clark, of New York.

But while Dr. Guyon has remained comparatively silent on the morbid changes left in the tissues and organs by the disease under consideration; and we know as yet little or nothing of the researches of the native physicians; Dr. Lyons's excellent report presents us with a most valuable record of his ample investigations in the matter. Nowhere, indeed, shall we find a more minute and comprehensive account of the pathological anatomy of the disease. It embraces a detail of the lesions occurring in the several tissues and organs implicated, primarily or secondarily, in the complaint—in their solids as well as in their fluids. We have not the space required to enable us to follow Dr. Lyons in these details, and to do full justice to his statements on the subjects involved in the inquiry. Nor, indeed, do we deem it necessary to enlarge on the subject, even were it in our power to do so without inconvenience, inasmuch as the lesions recorded in the work before us are, for the most part, similar to those noticed elsewhere in the same disease, and rather confirm what has been noticed by other investigators, than indicate the existence of pathological characters before unknown. On one point only shall we dwell somewhat at large, *i. e.*, the condition of the liver.

Dr. Lyons states that the most remarkable, the most constant, and, to his mind, the most inexplicable condition presented in the *post-mortem* examination of fatal yellow fever cases, was the state of that organ. He believes it may be affirmed that *some* departure from its normal state was an absolutely constant condition in all the cases which proved fatal. In relation to colour, firmness, consistence, dryness, the organ presented the characters recognized by all morbid anatomists, especially since the researches of Louis in the Gibraltar epidemic of 1828; while the microscopic examination of the hepatic tissue, which Dr. Lyons instituted, fully confirmed the results obtained by Dr. Clark, and subsequent pathological anatomists in this country.

"The fawn-yellow coloration," Dr. L. remarks, "usually well indicated the change that had taken place in the hepatic tissue; but it was not only in those cases in which the liver presented this coloration that abnormal states of the hepatic structure existed. The yellowish-brown colour, 'chocolat au lait,' was attended with similar, and fully as well-marked changes. The same may be said of the nutmeg condition of the organ, and even to some extent of several cases in which the ordinary liver-brown colour was unchanged.

"Minute fine sections (by Valentin's double knife), or matter scraped from the hepatic texture, exhibited the hepatic cells filled with globular oily and fatty matter. The natural appearance of the cell was completely altered, its outlines obscured, and its nucleus rendered invisible. It was surcharged with molecular and globular oily matter, while the whole field, and the interspaces

[1] Dutraulau, Topographie Médicale des Climats Intertropicaux, p. 98, foot-note.

between the cells, were filled with similar and equally abundant oily and fatty elements. When carefully treated with ether, its oily and fatty elements were in part dissipated, and the contour of the cells brought more clearly into view, but it was only rarely that the nucleus could, even by long treatment in ether, be made visible.

"It was sufficiently obvious from these investigations, that the marked character of the hepatic lesion was that of fatty degeneration, with accumulations of fatty elements in the otherwise normal hepatic cells."

Feeling convinced that researches with the microscope, however carefully made, gave but imperfect and unsatisfactory determinations of the positive and comparative amount of fatty deposit in the hepatic tissue, Dr. Lyons determined to estimate the specific gravity of the organ in a good many cases, with a view to a more accurate appreciation of the amount of change thus induced; it being of course fairly presumable, as he thought, that in proportion of the amount of fatty accumulation in the hepatic structure the specific gravity of the organ would be found diminished accordingly.

"The general results were uniformly found to bear out this presumption. It was ascertained that in general the fawn-yellow colour of the liver corresponded with abundant granular and molecular fatty matter in the hepatic cells, as shown by microscopic examination, and with diminished specific gravity of the liver substance, as shown" "by the areometer of Beaumé. The results thus obtained were further confirmed by the determination of the absolute quantity of ethereous extract in given weights of hepatic substance in different cases."

"The mode of procedure was as follows: A very strong solution of common salt[1] was made in a glass vessel of suitable height. The areometer of Beaumé was allowed to float freely in this vessel. Small cubes of hepatic substance were cut from the central parts of the liver, and immersed in the fluid; if they sank an additional quantity of salt was carefully added; if they floated high upon the surface, water was gently poured into the solution. In either case, the precise moment was carefully observed when the cube of hepatic substance, after having assumed a position between floating and sinking, and having oscillated slightly up and down, seemed for an instant or two to be in equilibrium; the degree marked upon the areometer was then recorded as accurately as possible. But as the instrument marked only whole degrees, the readings of fractional parts of a degree could not be taken with all the accuracy desirable. In recording the observations, care was taken to allow the error of sight to be on the side of the greater rather than the less specific gravity."

Dr. Lyons next proceeded to determine the absolute quantity of fatty matter in the liver by maceration in sulphuric ether and subsequent evaporation. The results were generally conformable to what was already stated. They prove that with the yellow or buff-coloured, or even the chocolate-coloured state of the hepatic substance in the cases examined, there was found a considerable quantity of that matter, and that, in some instances, a very marked increase of the fatty element was determined. It has already been shown that at the microscope and the areometer a corresponding result was obtained.

Some examinations, both qualitative and quantitative, were made to determine the state of the saccharine function of the liver. The following general results were obtained:—

"In cases fatal within the first period of the disease, the hepatic tissue gave the characteristic saccharine reaction with the Barreswil fluid, cupro-potassic solution. In cases of longer standing, which had passed into the second or third period, the reaction with the Barreswil solution was extremely faint, and in some instances was altogether absent."

[1] This, though not the most suitable agent for estimating the specific gravity of animal tissues, was the only one readily available.

In some instances Dr. Lyons obtained slight but appreciable reaction with the liquor potassæ test, with the production of a faint but characteristic odour of caramel.

Similarly also to what has usually been observed in most places and in the greater number of epidemics, the fever, though not sparing any classes of individuals, manifested a special preference for persons of a strong and plethoric constitution and enjoying robust health. It occurred more generally and was more fatal among males than females, as well as among individuals in the prime of life; while it spared, comparatively at least, young children, old people, and valetudinarians. All this is conformable to what was observed in the same city in 1723, and has almost invariably occurred elsewhere. The state of marriage exercised an influence on the mortality. Of 3,486 unmarried persons 1,123 died, or 32.21 per cent. Amongst 1,099 married persons 498, or 45.31 per cent. died; and of 503 widowed (276 females), 263, or 52.43 per cent. died.

Of 1,192 vaccinated patients 351 died, or 29.44 per cent.; and of 2,308 non-vaccinated 894 died, or 38.73 per cent. Of 2,087 who had had variola 635, or 30.42 per cent. died; and of 1,279 who had not had variola 556, or 43.54 per cent. Unlike what appears to have happened during the last-mentioned epidemic when, as we are informed on the authority of Sanchez, the black race was completely spared, a goodly number of persons of that colour were attacked with the disease in 1857. This liability of negroes to the fever, though contrasting with the results observed in former times at Lisbon and in most other places, from the earliest records of the disease, cannot be regarded, at present particularly, as a subject of surprise, inasmuch as, independently of the circumstance that in extra-tropical climates, where natives or long residents are never secure unless through the effect of an attack, individuals of the black race, though suffering in a less proportion and with less fatality than whites, have seldom been found to pass unscathed through an epidemic of considerable violence. It has not unfrequently happened, indeed, that they have furnished no inconsiderable quota to the list of cases and deaths, even in places where they had heretofore been regarded as perfectly secure.

The disease spread widely and fatally among the upper classes of society, contrasting in this respect with what had taken place the year before in the same city in regard to the Asiatic cholera, which caused a mortality of more than 4,500 persons, principally, if not wholly, among the lower classes. Hence, while the latter disease received from the inhabitants the epithet of democratic, the yellow fever was held up as being aristocratic in its tendencies.

In the worst quarters of the city the mortality was 42 and 43 per cent., the mean mortality being about 33 per cent. The mean duration of the sojourn in hospital was 6 days; for those cured it was 8 days; a little longer for women than for men. In fatal cases the mean duration was 4 days; for men a little more.

The number of physicians and surgeons practising at Lisbon and its dependencies at the time of the epidemic amounted to near 240. Of these, according to one report, 14 died, or 1 in 17.14; and according to another 18, or 1 in 13.34. This loss, though heavy, is small in comparison with that often experienced elsewhere. In this city, where the physicians were not, any more than they could be at Lisbon, acclimatized to the disease, the mortality among them during some of our epidemics was much more considerable.

In 1793 the number of physicians, regular and irregular, enumerated in the

Directory as residing not only in the city, but in Southwark and the Northern Liberties, was 61. In 1797 it amounted to 68, and the next year to 75. In the first of these years, exclusive of medical students, no less than 10 physicians were swept off by the fever in little more than a month. We are further told that hardly one of those that survived or remained in the city escaped an attack. This gives us a proportion of about 1 in 6 of the whole number. In 1797, 9 fell victims to the disease, while of the survivors 8 were affected more or less severely. This gives a proportion of 1 death in 7.55, and 1 case in 4. New York, in 1798, lost 11 of her physicians out of probably some 30 or 40, of which the medical corps was then composed. The same year Philadelphia, more fortunate than she had been on preceding occasions, lost but 3, besides 2 medical students, or 1 in 25 of the whole number.

It must be remembered that while the number of physicians in the city and districts was such as mentioned, the actual number exposed to the disease was much more limited. We are told by Dr. Rush that at one period of the epidemic of 1793 there was a great deficiency of physicians, from the desertion of some and the sickness and death of others, and that at one time there were but three physicians who were able to do business out of their houses. To this may be added that many lived and practised at a distance from the infected districts, and probably never approached the sick. In 1797, as we again learn from Dr. Rush, the disease bore, not on 68 physicians, the number registered in the Directory, but on only three or four and twenty, this being the full amount of those who attended patients in the disease. The others had probably deserted; or kept aloof from the scene of infection.

In 1798 this city could boast, at the time of the epidemic, of only 28 physicians doing duty among the sick. The loss, therefore, comparatively limited as it was, gives a larger percentage than was noticed at Lisbon. At Martinique, in 1821, of 15 unacclimatized and unsecured physicians 10 perished. Four years after the loss was still greater; for 3 out of 4 doing duty among the troops fell victims to the disease. In Senegal, in 1830, all the navy surgeons suffered to a greater or less extent, and 6 out of 12 died.

The apothecaries suffered more extensively at Lisbon than the physicians. They numbered only 135, and yet sustained a loss of 13, or 1 in about 10.4. Their proportional loss was therefore nearly double that of the physicians; for had their number equalled that of the latter, the deaths among them would, in the same ratio, have reached 23.11.

The fever, as is usual in extra-tropical climates, commenced and was most prevalent in the lower parts of the city, and produced, as we have seen, its greatest ravages in those localities specified, and the streets particularly remarkable, for the abominations referred to. Spreading at first along the borders of the river at each side of the spot where it broke out, it subsequently extended to the upper parts of the city. Though passing, as it were, gradually from house to house, it often left, in its onward course, one or more intermediary buildings free from its presence. The same may be said of its mode of progression from street to street, as also from district to district. In other words, it respected one or more streets and districts situated between those attacked. In none of these instances was it possible to explain the cause of this exemption; the houses, streets, and districts which remained free from the infection presenting the same hygienic conditions and being placed under the same atmospheric influences as those in the vicinity. Several public institutions—the poor-house, the orphan and insane asylums,

the gas-works, &c.—though located in infected quarters, remained free from the epidemic visitation.

The disease, when it once broke out in a house, seldom failed to attack several of the inmates. As many as 14 and 18 persons were seized by it in a single domicile. When once a house became the seat of infection, it was rendered by the fact a focus of reproduction of the disease. It was dangerous to visit, and especially to lodge in it, whether or not it contained persons affected with the disease. The danger was especially great at night. In several instances it was observed that infected houses could be visited or resided in during the day with perfect impunity, while individuals who remained there at night were almost sure to suffer. The same danger from night exposure was exemplified in the city at large, after the disease had assumed a firm footing in it and become general. Persons who, residing in the country, came to town during the daytime, and returned home in the evening, seldom were affected. In this respect the yellow fever of Lisbon bore the closest analogy to the disease as it shows itself in this and other countries, in relation to which the danger of night exposure has invariably been signalized. Well, indeed, may it be said that on no subject connected with the etiology of the yellow fever are writers more unanimous than in attributing to night-air in an infected district a more baneful influence than to that of day. Of the large number of such writers, scarcely one can be found unprepared to admit that exposure to such districts by night is almost sure to be followed by an attack.

In another respect the disease presented a phenomenon similar to that observed, if not always, at least frequently, in this country and elsewhere—its inability to spread beyond the circumscribed sphere of the infection. Developed within the limits of the city, it was not conveyed by emigrants who had been exposed to the influence of the poison, or by the sick, to the environs or to the interior parts of the kingdom, although many of the former sickened and some died after their removal, or left the city already labouring under the premonitory symptoms of the fever. Such occurrences, common enough, as we all know, everywhere, had already been observed in Lisbon; for we learn from a statement by Kennedy and Leitão, relative to the epidemic of 1723, that the high parts of the city were much freer than the lower parts, and that the villages and country houses of the vicinity were entirely exempt from the distemper, notwithstanding the utmost communication was kept up between them all the time. Da Cunha makes similar statements as regards the localities most affected, adding that "it was only in the unclean places that there was this multitude of diseases." Of the epidemic of 1724 kindred remarks are made by José Pinheiro de Freitas Soares.[1] "It was thought to have its origin in the corruption of the air by reason of the dirtiness of the streets, because the epidemic attacked chiefly the families that lived in the lowest parts of the city, where the streets were very narrow and dirty, and in the higher places it was only a few families that were attacked."

Dr. Lyons notes the immunity enjoyed by the shipping in the Tagus, notwithstanding the great and most constant intercourse with the town. He knows of but one instance of a British seaman (master of a brig) having become a victim to the disease. The individual in question had been drinking on shore.

As is usually the case in extra-tropical regions, and as was observed in 1723, the epidemic of 1857 broke out after, and continued to spread at first during, the prevalence of very hot weather. A hot, humid atmosphere

[1] Policia Medeca, 1818, p. 349.

tended to increase its extension and virulence. The pressure of the atmosphere exercised an equal influence upon it in both respects. It increased with the depression of the barometer, while a change for the better was observed as the pressure diminished. But although the fever appeared under the influence of high atmospheric heat, it continued to prevail, more or less extensively, after the temperature had greatly lowered. On the 1st of December, some seven or eight weeks prior to the final cessation of the epidemic, the thermometer marked 13°.6 of Cent. (56°.4 of Fahr.), and on the 31st 7° (44°.6 Fahr.),[1] and yet cases occurred, though in diminished number, for some weeks after. Kindred results had already been noticed at Lisbon in 1723, and are referred to by Dr. Kennedy, who, in the letter several times quoted, mentions that the weather had changed several times to rain and cold during the course of the epidemic, without any abatement in the distemper. It may, however, be presumed that these changes, though sufficiently marked to be noticed, were not so great as to bring the thermometer down to the freezing point, an event which at the close of October never occurs at Lisbon. To those of our readers who have devoted some attention to the history of the disease, the fact just mentioned will not be a matter of astonishment, for this continued occurrence of cases, after a change of temperature has taken place, is frequently noticed. Thus, to speak only of the events in this country, it is a well-known fact that when our cities are visited epidemically with the yellow fever, the disease is not always particularly rife during the hottest months of the season, but some time after, when the average temperature has lowered in a notable degree; and that, so far from its diminishing as the weather becomes cooler, cases are often multiplied daily till the temperature reaches the freezing point, when the occurrence of new cases ceases at once. Take the epidemic of 1793 in this city as an example. The deaths in August, when the mean temperature at mid-day was 82°.55 (69°.03 in the morning), amounted to 325; in September, with a mean heat of 76°.48 (60°.93 in the morning), the mortality reached 1,442; and in October, with a temperature of only 62°.57 (44°.06 in the morning), the deaths fell but little short of 2,000.

Cool weather, therefore, when the disease is fairly established, so far from diminishing its extension, sometimes increases it. These facts have been noticed not only here, but in our Southern States also, as well as at Barcelona in 1821, and in tropical climates.

While the yellow fever thus prevailed in Lisbon, and was exercising there its usual ravages, the country around was sorely afflicted with malarial fevers of the remittent and intermittent types. They extended their sway to the very gates of the city, and prevailed in all the localities situated along the Tagus, as far as Santarem. These fevers are of annual occurrence, in the same season, in these localities, but were uncommonly rife in 1857, in consequence of the large number of men collected there for the completion of the railroad connecting Lisbon with Santarem.

We do not find that anything of importance was done in relation to the treatment of the disease. At an early period of the epidemic many of the physicians resorted to the lancet, but the results were not such as to encourage them in persevering in the practice. It was, therefore, finally abandoned. More success appears to have been obtained from the use of stimulants, counter-irritants, purgatives, and the employment of special remedies, such as bark, iron, and more especially quinia. The majority of physicians, though differing relative to the precise time at which quinia

[1] If the 13°.7 and 7° indicate degrees in the scale of Reaumur, the equivalents in that of Fahrenheit are 62°.6 and 47°.75.

should be administered, thought highly of its effects. Dr. Lyons, in speaking of the remedy, says it was used both by the mouth and anus. Given by the mouth, it was prescribed in large doses; and in small doses it was used at the outset of the disease, and in all stages to the last. In the form of enema he saw it administered to the extent of seventy-two grains, divided into four clysters, one of which was given every sixth hour. From his experience of quinia, as administered in the epidemic, he is far from being disposed to regard it as a drug upon which any reliance can be placed when exhibited during the course of the disease. He is of opinion that the indications for its use as an antiperiodic are not very clearly manifested, and that its tonic action is too slow in a class of cases in which prompt support to the system and immediate stimulation are often so urgently called for. It would appear, however, from what Dr. Guyon observed while in Lisbon, that the remedy was found more useful when employed at the period of remission usually occurring in the morning during the first stage of the attack, than during the metaptosis, or that moment of calm constituting the link between the first and second stages of the disease, and which some pathologists wrongfully regard in the light of a true remission. It may be presumed, from what is here said of morning remissions during the first stage, and the benefit derived from this antiperiodic when administered at these periods, that the disease was generally modified through the influence of the paludal poison, which, as we have seen, was rife at the time, and must have penetrated, to a greater or less extent, to the very heart of the city.

Dr. Lyons informs us that the perchloride of iron was employed as an internal remedy, in doses of from three to six grains, two, three, four, or more times in the day, and, in some cases at least, with apparently good results, in arresting the hemorrhagic tendency. Chromic acid, in similar doses, was also used internally with the same view, but, as he thinks, with less advantage than the iron.

Dr. Guyon furnishes us no satisfactory information upon which to base an opinion respecting the probable origin of the fever and its mode of propagation. He himself hazards no conclusion, and postpones to some future time the publication of the facts he has collected on the subject. It would be unsafe, therefore, could we appeal to no other authority than his own, to arrive at a decision on the important points in question. Much to our gratification, Dr. Lyons has been less disposed to withhold the opinion he has formed on the subject. More particularly pleased are we to find that the occurrences in Lisbon go far to confirm us in the views we have long entertained respecting the local origin and non-contagious character of the yellow fever.

Dr. Lyons remarks that, from the first invasion of the epidemic, a belief became wide-spread and general, that the disease had been imported. This opinion was shared alike by persons in all ranks of society, by many amongst the educated, as well as the illiterate, and by many highly respectable members of the medical faculty, as well as by the public at large. It was not, however, universally accepted, for both in the faculty of medicine, and from several non-professional persons of great intelligence, the most distinct avowals of a belief in the entirely local origin of the epidemic were made, while certain existing conditions were pointed out, manifestly sufficient to account for its causation. As an impartial investigator, Dr. Lyons considered it to be his duty to inquire very fully into such evidence as was adduced in support of both sides of this important question. On this subject he remarks:—

"After a most careful inquiry amongst various official persons, and in quarters in which reliable evidence could be expected on such matters, I am obliged to state that in no one instance did I obtain such a consistent assemblage of facts, or such an array of well-supported allegations, as would, in my mind, warrant the conclusion that the importation theory was even moderately well founded. On the contrary, so vague, and in some essential particulars, so conflicting were the allegations as to the time, place, and other circumstances attending the reputed importation of the disease, that I do not believe that the popular opinion so generally held as to the importation of the epidemic admits of being reduced to anything like a uniform or consistent statement in writing.

"Of the reports in circulation as to the alleged importation, the two most generally received were, firstly, that the disease was communicated to persons in the custom-house, engaged in the examination of the baggage and personal effects of passengers arriving from the Brazils; secondly, that the manner of importation was by certain cargoes of hides from the Brazils. In connection with the first report, it is to be observed that there was no uniformity in the accounts given by different persons of the circumstances of time and place under which the importation was alleged to have occurred. There was no concurrence of testimony as to the particular ship, the date of her arrival, the port she sailed from, or the health of the passengers she brought. By one set of persons the ship specified was the Brazilian mail steamer Tamar; by others, a different ship was particularized with equal confidence.

"In one account of the importation of the disease, in connection with hides from the Brazils, it was stated that the hides were landed at the custom-house; in another, that the hides were landed at a certain wharf higher up the river. After the most careful inquiry in all quarters, I am unable to produce any more circumstantial account of the alleged importation of the malady than is contained in the foregoing statement.

"The considerable number of attacks and deaths which took place in the custom-house, and in the adjacent parts of the city, was constantly adduced in proof of the origin of the epidemic in the custom-house, and its propagation from that establishment as a centre." No such argument, however, "is supported by the facts. The undoubted frequency of the attacks, and the mortality in the quarter in question, were equalled and surpassed in districts considerably removed from proximity to the river."

But besides all this, many things—nearly all of which are adduced by Dr. Lyons—point to the conclusion that the epidemic in question was of local origin, and devoid of contagious properties. In the first place, we may remark that the epidemic which broke out at Lisbon in 1723, and carried off, as we have seen, 6000 persons, was never shown to have been introduced into that city from abroad. Nor was it held, at the time, by those competent to decide, as being propagated by contagion. We have seen that, according to Leitão, there were "most deaths where most dirt," when vapours arose by reason of heat, and the winds which prevailed were from the west. We have seen, too, that Soares says of the fever of 1724, that it was thought to have its origin in the corruption of the air, by reason of the dirtiness of the streets. Writing a few years later of what he had noticed on the memorable occasion of the epidemic of 1723, Da Cunha says: "What I always said, and which I persist in, is that diseases were not contagious, and they were all choleric diseases, caused by the heating and exalting of the cholera (bile), all occasioned by the inclemency of the weather." He admits that others were not of his opinion, and that various objections were raised to his views. But he adhered to these, and upheld the doctrine of local origin and non-contagion, by reasonings and a display of facts that would do no discredit to a modern infectionist, while his antagonists contented themselves with stringing together unsupported assertions, and could never point out the source of the alleged importation,

or a proof of transmission from the sick to the well. In this connection, the following passage is interesting.

"I remember that, being physician in Alfizerâo and St. Martin, there was a lake between the two districts, which, when the summer came on, dried up, and part of which remained putrefied, and the winds coming, carried the vapours to both districts; and there were so many diseases, that scarcely one person escaped without falling sick, having myself 125 persons sick, and almost all with malignant diseases; many had parotitis and pinta. The winter coming on, the lake was inundated again, and all the diseases vanished; and the following year the lake opening itself, and what remained being burned, there were no more similar diseases."

In another place, after stating that it was only in the unclean localities that this multitude of cases occurred, he says: "The sun causing the more subtile particles to rise in vapours, which entering into the houses were introduced into the body, which was prepared to receive them by the rarefaction of its pores."

We are not unmindful of the fact that the epidemic in question was said by Sir Gilbert Blane to have been the result of importation from Brazil. But the mere assertion of that writer cannot be admitted as sufficient to establish the fact, inasmuch as he does not state where and from whom he obtained the information; and writing as he did, a full century after the event, he could not know much about it himself. A little reflection might perhaps have led him to refrain from making the statement, for the fever did not prevail in Brazil at the time, and could not very well be carried thence across the ocean. But importationists and contagionists of pure water, as Blane was, and as many others we could name are, have an easy way of getting over difficulties of the sort; and when we recollect that the epidemic of Cadiz, in 1819, was by some, otherwise clever men, traced in a way satisfactory to them, to a vessel which some months previously had arrived from Calcutta, where, as is well known, the yellow fever never shows itself; when we bear in mind that, to meet an objection raised against that theory, and founded on the invariable absence of the disease, at its supposed source of origin, it was contended that the Asiatic cholera, which *was* prevailing there at the time, might well be supposed to have been transformed by change of climate into the yellow fever; when, besides, we are gravely told that the epidemic of Agamont, in Spain, owed its origin to a piece of cheese which some fishermen had procured at sea from an English vessel of war on her passage from Gibraltar to England, which vessel they did not board, contained no sick, and did not communicate the fever at the port where she arrived; when we recollect these things, we say, we must be prepared for any statements by which physicians of the category mentioned may uphold their favourite theories. Let it be remarked, that nothing has been said of the foreign origin of the fever of Ericeira, in 1721; of Lisbon, in 1724; of Peniche, in 1728, and of Funchal, in 1738. Again, let it be remarked, that if the epidemic of 1857 is to be attributed to importation from abroad, the same explanation must be given relative to the fever which broke out, not only at Lisbon, but at Belem also, in 1856. Now, how did matters stand, in relation to that epidemic?

Of 311 cases recorded to have occurred in Lisbon in 1856, somewhat more than two-thirds were observed in districts far removed from the river margin. The three districts attacked in 1856, Belem, St. Roque, and Los Anjos (Bica) are widely separated from each other, and present no recognizable connection, commercial or otherwise, among themselves, nor with

the exterior by way of sea traffic. These districts are likewise on different elevations.

Let it be remarked, too, that the yellow fever of Oporto, in 1856, is attributed by Dr. Gonveia of that city to the filth of the quarter Miragaia, in which the disease prevailed, and not to importation; and no one has yet been able to disprove that opinion.

Nothing in all this looks as in any way favouring the opinion of the disease being of foreign origin. Add to it that, so far, no fact has been adduced likely to justify, in the most remote degree, a belief in this origin. No vessel or individual has been pointed out with any show of probability as being the agent of introduction of the disease. Are we not at liberty to say that until those several visitations are shown to have been the result of an imported infectious poison, the more violent and disastrous epidemic of 1857 may well be considered as being produced by local causes? Dr. Lyons calls attention to the fact that while there is no uniform testimony as to the time, place, and other circumstances of the alleged importation of the epidemic in 1856 and 1857, it is certain that no case of the disease was received into the lazaretto in either of those years, or in any recent year. (The Inspector of this place, who has resided there forty-two years, affirmed in the most positive manner that there has never been one single person of those undergoing quarantine who was attacked with an epidemic disease.) The fact of the fever not being communicated out of the city by emigrants in health, or by those who afterwards sickened and died in the country, certainly does not indicate the existence of contagion. On this subject the statements of Dr. Lyons are positive.

"The freest communication was kept up, even during the height of the epidemic, between Lisbon and Cintra, and many other favourite places of resort, to which the citizens retired in great numbers; yet no cases can be adduced to show that the disease spread or was carried by contagion or otherwise from Lisbon to such localities. Cintra was perhaps the place most frequented, and with which much free daily communication was consequently kept up; but I could obtain no reliable evidence that one single case of genuine yellow fever occurred in that town. I have heard statements," Dr. L. continues, "to the effect that the disease was propagated by contagion to some minor towns, but I have seen no evidence, documentary or otherwise, to prove the truth of such allegations, or render them even probable. On the contrary, the uniform testimony of popular report (so far as it is at all reliable or worth noticing) went to show that the disease was not propagated beyond the walls of Lisbon."

As already mentioned, no restriction seems to have been imposed to the intercourse between the shipping on the Tagus and the inhabitants of various parts of the city, or between the sick and well, wherever the former might be; and again, as we have seen, the inmates of many houses, though in close proximity to others containing persons labouring under the disease, and doubtless visiting these, remained free from its attacks throughout the whole duration of the calamity. The inhabitants of certain healthy streets or quarters of the city, although visited daily by individuals who had been exposed, by residence or otherwise, to the poison in infected localities, and whose clothes may well be supposed to have been impregnated with such poison—although themselves mixing, for traffic or otherwise, with the population of the sickly parts, remained free from the disease, or if they took it there, did not communicate it to their families, attendants, or visitors.

It is not customary for apothecaries to mix much with the sick. They, as a general rule, remain in their shops. While they do this, physicians, as a necessary consequence of the nature of the duties they are called upon to perform, approach and touch the sick, and are in every possible way exposed

to the baneful influence of contagious poisons, when such poisons exist. Nevertheless, as we have seen, apothecaries suffered in a far greater proportion than physicians. It may be remarked, in corroboration, that a class of females who, from their frequent and close personal intercourse with strangers and individuals exposed to and carrying about them the contagious poison—we allude to prostitutes—were scarcely touched by the disease. They numbered 600 at the time of the epidemic. Of these only 5 or 6 died, or 1 in 60. In addition, attention may be called to a fact already adverted to, that individuals from uninfected spots, or from the country, who visited less favoured localities, escaped the disease, provided they retired to their places of abode before night. They might enter and perambulate the city, for business or otherwise; they might step from their healthy streets and districts to those in which the fever raged with violence, and enter and remain for hours in houses actually occupied by the sick, or where deaths in greater or less number had but recently occurred; they might, indeed, visit the very chamber of the sick and dying; but they must do so before nightfall. At a later hour such exposures became dangerous, and those who slept all night in such places were almost sure to be seized with the disease.

It may not be improper to add, to all that has already been said, that instances of immunity from the so-called contagion were long ago observed at Lisbon. Kennedy states that at the time of his writing—31st of October, 1723—two months and a half after the outbreak of the fever, the latter had not yet entered any convent except the Irish, although the inmates of all those establishments never intermitted their attendance on the sick. We must also remind the reader that the same physician stated that during this epidemic of 1723 the villages and country-houses about town were entirely free from the disease, "notwithstanding the great communication."

The reader must bear in mind that all the parts of the city largely attacked by the epidemic present in common certain conditions of insalubrity, which have been described in detail in an early part of this article; nor must we omit to mention that the state of the shore of the Tagus, for fully one-half of the extent of the city along the river-side, is such as to be a highly effective cause of insalubrity, not only to the adjacent districts, but to the whole capital. The vast area of extremely fetid decomposing mud left exposed at low water exhales noxious gases very prejudicial to health.

While in the epidemic of 1856 two out of the three localities chiefly attacked—St. Roque and Bica—were at some distance from the river, in 1857 the largest number of cases occurred in the first instance in the proximity of the custom-house, and in that establishment itself.

"No case," says Dr. Lyons, "is recorded in this locality prior to September, 1857; but before this date there seems good ground for believing that cases of undoubted yellow fever had been observed. One case is recorded as having been verified so early as May, 1857. Another case is stated to have been seen in July, while there appear to have been at least five in August. Some of these cases occurred in the quarter St. Roque, an elevated district, 100 to 150 feet above the Tagus level." "It has been attempted to give a special significance to certain of these cases, by saying that the parties attacked, though residents in the district of St. Roque, were employed in the custom-house. I can neither affirm nor positively deny the truth of the statement, as I could procure no reliable evidence respecting it, and I am not aware that any such exists. My own conviction, based on the results of inquiries made amongst medical men of all shades of opinion, is, that prior to the declared and fully recognized existence of the epidemic, isolated cases presented themselves in various parts of the city, of which no accurate record was made, but of the existence of which no moral doubt can be entertained."

In conclusion, it may be remarked that although public opinion was strongly in favour of the importation theory, little if any apprehension of personal contagion was entertained by those in attendance upon and in daily contact with the sick. This indifference to exposure to the supposed contagion of the fever was observable in all classes of society, amongst the lowest as well as the highest. A large number of the inhabitants, it is true, sought safety by flight from the focus of the infection; but amongst the population which remained, "humanity was spared those humiliating and appalling scenes which the medical historian tells us were so constantly presented in the epidemics of the middle ages, of the sick and dying abandoned by their nearest relatives and friends." R. La R.

ART. XVI.—*Chemistry, in its Relations to Physiology and Medicine.* By GEORGE E. DAY, M. A., Cantab., M. D., F. R. S., Professor of Medicine in the University of St. Andrew's. London: H. Baillière, 1860. 8vo. pp. 518.

THE department of science to which the treatise before us relates has, through the labours of its numerous cultivators, become so intimately connected with rational practical medicine as to exercise a very material influence upon the views of those physicians who act in accordance with facts rather than fancies, and, as a consequence, has modified to a considerable extent the practical application of medical principles.

We need not stop now to discuss the opinion above enunciated, as it will, we think, be sufficiently established by what we have to say in detail of Dr. Day's volume; and yet we can scarcely refrain from calling to mind how the students of physiological chemistry have been reviled; how they have had to contend with many of those who, from their eminence and ability, should have opened their minds to the light of truth, but who, alas! have wilfully or ignorantly closed them against the mild persuasions of science. We cannot forget how many prejudices have been called into action against those who brought to the investigation of nature's laws the rigid tests of experimental analysis, and how even the sacred influence of religion was sought to be hurled against those who presumed to employ the principles of chemistry in their inquiries concerning the functional actions of the organism. We do not forget these things, and, remembering them, we find the greater cause for congratulation that, in spite of those obstacles which since the inauguration of the freedom of thought have always been opposed to the progress of truth, the science of physiological chemistry has steadily, though slowly, advanced till it has attained a recognized position among the sciences developed by the reason and the labour of man. True, there are still some who refuse to receive its teachings; there are still some who prefer the vague theories of the past to the facts of the present; but their name is *not* legion, and their voice is *not* mighty, and they grope confusedly in the darkness they love so well, vestiges of an age which, we trust, has passed away forever.

The treatise which we propose to bring before our readers in this notice is divided into three books. Of these the first treats of "*The Organic Substrata of the Animal Body; the Proximate Principles entering into the Composition of the Solids and Fluids of the Organism.*" Although it is essential, for the full understanding of the science of physiological

chemistry, that a full acquaintance should be had with the subject-matter which Dr. Day has incorporated into this division of his volume, we do not propose to dwell upon it now. We desire, however, to call attention to a few of the more notable substances which are treated of under the above head; and after having done so, will proceed to consider the more interesting, if not more important, subjects which follow.

In regard to the protein bodies, Dr. Day includes under this head albumen, fibrin, syntonin, casein, globulin, and hæmatocrystallin, as being all that are met with in the animal organism. These substances occur under two chief forms—in solution and coagulated, but are only met with in the animal body in the first-named condition. Most of them may be converted into the insoluble form by heat, by the mineral acids, or by saline solutions; and fibrin assumes this form on being removed with the blood in which it exists from the body.

It is frequently important that the physician should be able to determine the presence or absence of protein bodies; and in order to do this, Millon's test is more generally to be relied on, both on account of its delicacy and certainty. It consists in the employment of a solution of mercury made by dissolving one part of mercury in two of nitric acid of specific gravity 1.41. On the addition of this solution, and raising the temperature of the mixture to from 140° to 212°, an intense and persistent red colour is produced. The only source of error is that the same reaction is given with gelatin and its derivatives.

Dr. Day also refers to the iodine test so much used by histologists in microscopical examinations, but gives no particulars in regard to its preparation and employment. One essential point is that the solution of iodine should not be too strong, or the reaction may be so marked as to interfere with the obtaining of definite results. We have found a good preparation to consist of from two to four grains of iodine and about half the quantity of iodide of potassium to the ounce of water. If the tissue under microscopical examination is subjected to the influence of this fluid, by introducing the latter under the edge of the thin glass cover, a deep brown colour is produced, if the former be of a proteinaceous constitution.

Undoubtedly albumen is to be regarded as the type of the proteinaceous bodies; but, nevertheless, it is not identically the same substance, as it is met with in the white of egg, in the blood, &c. The cause of the modifications which exist would appear to be due to varying proportions of alkaline salts present; for, as it is found in animals, albumen is always in combination with these substances, and consequently never exists in a perfectly pure state.

In regard to the position of *fibrin*, Dr. Day expresses no decided opinion, but appears to regard it as belonging to the class of substances undergoing progressive metamorphosis.

Casein is not only found in milk, but also in the yelk of the egg. Its admixture with albumen was until quite recently regarded as a distinct proteinaceous substance, designated as *vitellin*.

Globulin or *crystallin* resembles albumen in many respects, but differs with it in not coagulating at a temperature lower than 163°, and in one or two other particulars. Globulin is only found in the crystalline lens. The coagulable matter of the red blood-corpuscles is not, as was maintained by Berzelius, identical with it.

Hæmato-crystallin, the proteinaceous substance of the blood above referred to, differs from all the other members of the group, in being crystallizable, and in not being precipitated from its solutions by nitrate of silver,

chloride of mercury, chloride of tin, or basic acetate of lead. Its formula has not been established.

The blood-crystals so generally met with in the blood of fish, and some of the mammals, birds, and reptiles, and which, with a little management, can be obtained from most red-blooded animals, consist of this substance or its modifications. The forms of these crystals are exceedingly various, but are constant for the same species of animals.

Lehmann has made the discovery that hæmato-crystallin may be broken up into a nitrogenous substance, and a sugar resembling glucose. This fact has recently been brought forward by several German pathologists in explanation of the pathology of chlorosis, and as an argument to the effect that sugar possesses an important curative influence over this affection. Nothing is known of the origin of hæmato-crystallin.

Passing over a good deal of other interesting matter contained in this division of the work, we come to the second part, which treats of the "*Chemistry of the Animal Juices and Tissues.*"

Dr. Day divides the fluids of the body into the five following groups:—

"I. The *digestive fluids*, including the saliva, gastric juice, bile, pancreatic fluid, and intestinal juice; and to these we have added the consideration of the intestinal contents.

"II. The *blood and its allies*, including the chyle, the lymph, the blood itself, and the various transudations.

"III. The *fluids connected with generation and development*, including the seminal fluid, the milk, and the fluids of the egg.

"IV. The *excretions of the mucous membrane and the skin*, including mucus, the various sebaceous matters, and the sweat.

"V. The *urine*."

Under the first group the saliva is first considered; and after the chemical properties of the secretion from each gland separately, and of the mixed saliva, have been pointed out, the author takes up the question of their physiology. Dr. Day does not, we think, give sufficient prominence to the experiments of Bernard relative to the uses of the parotid, submaxillary, and sublingual saliva separately. Most of our readers are doubtless acquainted with the results which this physiologist obtained, showing that the secretion of the parotid gland is mainly of use in facilitating mastication, that of the submaxillary in assisting taste, and that of the sublingual in promoting deglutition. The influence of the saliva in causing the conversion of starch into sugar is affirmed by Dr. Day; and though there is no doubt that it does effect this change out of the body, and to some extent in the mouth, we doubt whether Dr. Day does not go too far when he says that it is almost universally admitted that the principal use of the salivary secretion is to promote the conversion of the amylaceous portion of the food into dextrine, sugar, and lactic acid, and thus to promote its absorption. This view is objected to by several of the most eminent physiologists of the world; and though there are some facts which seem to support it, there are others, again, which go to show that the power to effect the change in question is incidental, and of no practical importance whatever. The matter is not settled, and physiologists have still an ample field for research before the true action of the saliva in this respect can be ascertained.

The second section of this part is devoted to the consideration of the *gastric juice*. The chemistry of this secretion is first considered. In regard to the free acid, Dr. Day appears to regard it as being hydrochloric, frequently, but not always, associated with lactic. The view of Blondlot, that the acidity of the gastric juice is due to the presence of acid phosphate

of lime, is very properly regarded as untenable. In regard to this point, we think the mass of evidence sustains the opinion of the author, and that the essential acid of the gastric juice is the hydrochloric. We do not deny the general presence of lactic acid, but we think it is derived from the decomposition of the amylaceous and saccharine articles of food, and is not altogether a normal product in this situation.

Dr. Day's section on the *bile* contains all that is positively known on the subject. In regard to the uses of this secretion, he thinks it is mainly employed in the digestion of the fat. This, however, is not proved, and we are still at a loss to designate the exact purposes for which this fluid is required in the economy. It is certainly essential to life, but it is just as certainly unessential to perfect digestion.

In considering the *pancreatic juice*, no mention is made of Corvisart's investigations relative to the action of this fluid on nitrogenous substances. It would appear, also, that Dr. Day sides with the German physiologists in their views regarding its action on fatty matters, views which are in opposition to those held by Bernard.

Owing to the difficulty of obtaining the *intestinal juice* in a state of purity, it has not yet been satisfactorily analyzed. Enough, however, is known of it to warrant us in saying that it is invariably alkaline. The quantity secreted in a given time cannot be accurately ascertained. In its physiological action it resembles both the gastric juice and the pancreatic fluid—dissolving proteinaceous bodies, and converting starch into sugar. It does not possess the emulsifying power in so high a degree as the latter secretion.

Vomited matters, *intestinal gases*, and the *feces* are next brought under consideration, and the latest investigation relative to the composition, &c., of these substances is fully given. With reference to the latter, we find no mention made in the text of the very interesting observations of Dr. Marcet. In a note Dr. Day states that he has refrained from alluding to them because of their comparative incompleteness, and he merely gives some of Dr. Marcet's conclusions. If this is a valid objection, it is certainly applicable to many investigations, the details of which are not omitted in the volume under consideration.

In the next chapter the *blood and its allies* are dwelt upon at length, and the matter which Dr. Day has brought together under this head constitutes, in our opinion, one of the best portions of the book, though we cannot say that any information not already in possession of physiologists is communicated. The views expressed, however, are, in the main, such as are supported by the greatest body of evidence.

The physical characters of the blood are first considered, and then the morphotic constituents are brought under notice.

The existence of a cell-wall to the red corpuscles is affirmed by Dr. Day, and several arguments adduced in support of this view. It appears strange to us that some physiologists should deny so evident a fact. We do not find that any opinion is expressed relative to the origin of these bodies. We have for some time been of the opinion that they were a further stage in the development of the nuclei of the white corpuscles, a view first propounded by Wharton Jones. We have several times seen fully-formed red corpuscles contained in cells in the spleen with white corpuscles and free nuclei, and we believe that it is in this organ, and others of its class, that the change is mainly effected.

In regard to the colourless corpuscle, Dr. Day is of the opinion that it contains, besides its nucleus and granules, an albuminous solution. We

find that whenever there is an increase in the number of these bodies, there is an increase in the amount of fibrin in the blood. An intimate relation would therefore appear to exist between the two circumstances. We therefore are rather inclined to adopt the view which was held by Addison, that, instead of albumen, the white corpuscles contain fibrin, and that it is only found in this situation in the blood. There are several other facts which might be brought forward in support of this theory, not the least of which is that in the buffy coat of a clot the white corpuscles may be distinctly seen breaking up and giving exit to a fluid which immediately undergoes fibrillation.

Dr. Day does not in this chapter consider the question of the physiological position of the fibrin. A subject of so much importance could not but have received some elucidation from one of the known abilities and good judgment of our author. We regret, therefore, that he has not given his opinions in detail, and we do so the more from the fact that we are at present engaged in a series of investigations relative to this subject, and should be glad to have the benefit of his views. These investigations are as yet far from complete, but we think they authorize us in expressing the opinion that fibrin belongs to the class of substances produced by the retrograde metamorphosis of the tissues.

The blood is not the same fluid as it is met with in the arteries and veins, or even in the various special vessels of the body. The main difference between venous and arterial blood consists in the fact that the former contains less oxygen and more carbonic acid than the latter. It also, we think, contains more fibrin; at least our own researches lead us to this view, though it is one not held by all physiologists.

Dr. Day holds to the opinion that the menstrual blood contains no fibrin, a view certainly not correct. The reason why menstrual blood does not coagulate is simply owing to the fact that it is generally discharged slowly, and is so acted upon by the secretions of the uterus and vagina that coagulation is prevented. When, however, it is discharged rapidly, it invariably coagulates as does other blood.

The composition of the blood in disease is next noticed, and much matter interesting and important to the physician is given. If physicians would only act upon the facts which are here shown to exist, they would be enabled to treat many diseases much more intelligently than they are now managed. The diseases to which the blood is subject, and the modifications which it undergoes when other parts of the body are abnormally affected, seem scarcely to attract the attention of those who call themselves "practical men."

After considering the "allies" of the blood—the *chyle*, *lymph*, and *transudations*—Dr. Day passes to the *fluids connected with generation and development*. The other chapters of this section relate to *mucus*, the *urine*, *pus*, and the *solid tissues*. We are obliged to pass all these over with the simple remark that they are treated in a highly satisfactory manner, and that Dr. Day evinces how thoroughly he is acquainted with the subject considered.

The third book treats of the zoöchemical processes, those actions by which the organism is enabled to live and retain health; and these we propose to consider as briefly as is consistent with a fair detail of the principal facts which may be regarded as established, or supported by strong evidence.

Dr. Day, in the following classification, embraces those substances which are essential to the well-being of the organism, or we may say those substances which enter into the composition of the food of man:—

1. The protein bodies and their derivatives.

2. The fats (or hydrocarbons).
3. The carbo-hydrates—*e. g.* sugar, starch, &c.
4. The mineral constituents of the body.

In regard to this classification, we think it is as complete as any that can at present be made, and one that is far preferable to Liebig's, which, aside from its imperfect character, is altogether wrongly based. That above given is identical with one proposed by the author of this notice several years since. It commences the chapter devoted to the important subject of the *metamorphoses of the tissues.*

After considering the uses of albumen in the economy, and the transformations which it undergoes, Dr. Day passes to the second class—the fats; and as the views which are expressed in his remarks are those which are now generally held by physiologists, we quote a paragraph or two from the volume before us.

"The second great group includes the fats, whose various uses in the chemistry of the animal body have been already noticed. We have there shown that, in addition to their mechanical uses, they take an active part in the chemistry of digestion, and in all the processes by which the fluid nutrient substances are converted into tissues, and have likewise there and in other passages drawn attention to the fact that no animal cell or fibre is formed independently of the presence of fat. In connection with the importance of fat in the animal economy, we may adduce the well-known fact that the introduction of fat into the body (either as food or medicine) predisposes the organism to the formation of cells; a cell-formation of this kind, however, requires the concurrence of albuminous substances for the construction of the cell-walls. When the organism does not find in the food sufficient materials to form the investing membrane of the fat-cells, it borrows from the muscular fibre the necessary protein substance; and when this source can no longer be drawn from, the fat begins to accumulate in the blood and other fluids. From these facts, and from certain microscopic observations, a theory of cell-formation has been propounded, according to which each cell is primarily formed by the deposition of a thin layer around a minute vesicle of fat. We are not prepared fully to support this apparently simple explanation of the origin of a cell; but this at least is certain, that fat is always to be found in all highly cellular organs (as, for example, the brain and liver), and in all tissues, during the process of their development; pus and certain cancerous growths are rich in fat; the hair-bulbs present an active formation of new cells, and we find them imbedded in the sebaceous glands; the chyle, which always abounds in cells in various stages of formation, always contains much fat; the germ of the egg is surrounded by the fatty yelk-fluid; and numerous fat-globules are found in the muscular and other fœtal tissues." pp. 420–421.

We see, therefore, how much the experience of the present day is against the view of Liebig, that the fats serve only as heat-producing substances, and not as histogenetic materials. The fact is, that no such exclusive classification as that proposed by this physiologist can be based upon truth, as all substances taken as food contribute both to the formation of tissues and the maintenance of the heat of the organism.

The formation of fat in the system is next considered. The weight of evidence is decidedly in favour of the view that fat is not only formed from the carbo-hydrates (starch, sugar, &c.), but also, under certain circumstances, from albuminous bodies. Dr. Day next passes to the metamorphoses of the carbo-hydrates and the mineral substances (through the consideration of which we regret not being able to follow him), and then proceeds to the discussion of the function of *digestion.*

We find nothing new in this chapter, though it is an admirable *rêsumê* of what is known relative to the important function considered. Dr. Day doubts the correctness of Mialhe's view, that calomel is converted into cor-

rosive sublimate in the stomach through the action of the chlorides of the gastric juice. He bases his objections upon the experiments of Van Oettingen and Buckheim. The only way to definitely settle this question is by direct experiment upon animals with gastric fistulas. We have performed such experiments, and are enabled to say that corrosive sublimate is formed, if means are taken to obtain the secretion of a sufficiency of gastric juice. The availability of the various kinds of food is next considered, and then the function of *respiration* is brought under notice. The several facts which have been ascertained, and the various theories which have been propounded in reference to this process, are fully dwelt upon, and then the twentieth and last chapter, which is devoted to *nutrition*, is reached. In this chapter the nutritive value of the several articles of food is considered, and Dr. Day adduces several arguments showing the necessity which exists for a mixed diet. Another element, frequently overlooked, is the digestibility of the food. Thus, no matter how suitable an article may be, from its chemical composition, to subserve some of the purposes of food, if it is not capable of assimilation it is useless. For example, looking at the composition of gum, it might reasonably be concluded that it would form a valuable heat-producing material, and, from its ready solubility in water, that it would readily be absorbed from the gastro-intestinal mucous membrane; yet when experiments are instituted with reference to this point, it is at once perceived that the substance in question, from its indigestibility, is altogether incapable of acting in any sense as a food. Physicians, however, who disregard the teachings of physiology, and who look upon experiments as useless expenditures of time and labour, continue to injure their patients by giving them gum-water, under the idea that they are sustaining the failing powers of life, and lubricating the mucous coat of an inflamed gastro-intestinal surface. The chapter concludes with a statement of the influence of alcohol and tobacco upon the metamorphoses of tissue, in which the conclusions arrived at several years since by the author of this notice are given in full.

We must now bring our remarks on Dr. Day's treatise to a close. For the physician desirous of knowing what has been done by physiologists, during the last decade, to advance the science of medicine, we cannot recommend a better book than the one before us. Dr. Day is in a position to speak authoritatively upon all the subjects considered, for he has long devoted his attention to them, and few men have done more than he to extend a knowledge of the results which science has given to practical medicine. In perusing his writings, we never lose sight of his thorough honesty, and never fail to perceive how capable he is of giving a correct judgment. We would like to see his work widely distributed among the members of the profession, for we are confident that, were the facts which it contains brought home to the minds of physicians, we would have less empiricism, and might in time see the rise of that system of rational medicine, the advent of which is so much to be desired, but which is still, apparently, a great way off. That it will come, we do not doubt; and our confidence is increased when we reflect that those who labour in the cause are more than the equals, in ability, in eminence, and in numbers, to those who would force upon us the false and dangerous dogmas which spring from the effete systems of vitalism and solidism; and among those who have exerted themselves to break down the false and to erect the true, in the pure spirit of scientific zeal, we must assign a high place to the distinguished Professor of Medicine in the University of St. Andrew's.

W. A. H.

BIBLIOGRAPHICAL NOTICES.

ART. XVII.—*A Treatise on Human Physiology; designed for the Use of Students and Practitioners of Medicine.* By JOHN C. DALTON, Jr., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, &c. &c. Second edition, revised and enlarged. With two hundred and seventy-one illustrations. Philadelphia: Blanchard & Lea, 1861. 8vo. pp. 690.

UPON the appearance of the first edition of Dr. Dalton's treatise, two years ago, we reviewed it at length in our columns, pointing out what we considered to be its deficiencies, but dwelling at greater length, and certainly with far greater pleasure, on the many excellent features which characterized his work. We commended it for its originality, for its clearness, for the simple but elegant language in which it was written, for its beauty and fulness of illustration, and, with the exceptions which were mentioned, for its great adaptability to the wants of the student and practitioner.

Now, after the short period of two years, it comes to us again, "revised and enlarged;" and, with no intention of going over the ground which we formerly traversed, we enter with pleasure upon the congenial task of bringing before our readers the main points of Dr. Dalton's additions, and of again expressing our opinion of the volume which his learning as a physiologist and his ability as a teacher have enabled him to produce.

We cannot better indicate the character and extent of the additions which the author has made to his treatise, than by quoting the following extract from the preface to the second edition:—

"First, the introduction of an entire chapter devoted to the consideration of the *Special Senses*, which were only incidentally treated of in the former edition.

"Second, the rearrangement of the chapter on the *Cranial Nerves*, and the introduction of some new views and facts in regard to their physiology.

"Third, an account of some new experiments, original with the author, relating to the functions of the *Cerebellum*, and the conclusions to which they lead.

"Fourth, certain considerations respecting the general properties of *Sensation* and *Motion*, as resident in the nervous system, which are important as an introduction to the more detailed study of these functions.

"Fifth, the introduction of a chapter on *Imbibition* and *Exhalation*, and the functions of the *Lymphatic System;* including the study of endosmosis and exosmosis, and their mode of action in the animal frame; the experiments of Dutrochet, Chevreuil, Gosselin, Matteucci, and others on this subject; the constitution and circulation of the lymph and chyle; and, finally, a quantitative estimate of the entire processes of exudation and reabsorption, as taking place in the living body.

"Additions have also been made in various parts of the chapters on Secretion, Excretion, and Circulation, and the functions of the Digestive Apparatus. In every instance, these alterations have been incorporated with the text in such a manner as to avoid, as far as possible, increasing unnecessarily the size of the book.

"Twenty-two new and original illustrations have been introduced into the present volume; of which number, five replace others in the former edition which were regarded as imperfect, either in design or execution; the remaining seventeen are additional."

It will be seen, therefore, that Dr. Dalton's best efforts have been directed towards perfecting his work. The additions are marked by the same features which characterize the remainder of the volume and render it by far the most

desirable text-book on physiology to place in the hands of the student which, so far as we are aware, exists in the English language, or perhaps in any other. We use these strong expressions without, of course, intending to indorse all the opinions which Dr. Dalton expresses on physiological subjects. In fact, on the contrary, we find many where we are constrained to differ with him; but they are generally subjects which cannot be regarded as definitely settled, and where, therefore, a difference of opinion is allowable. We know no other treatise on physiology in regard to which we would not have to use similar language, and with more foundation for it than we have in the present instance.

We, therefore, have no hesitation in recommending Dr. Dalton's book to the classes for which it is intended, satisfied as we are that it is better adapted to their use than any other work of the kind to which they have access; and we trust that the speedy sale of the present edition will enable the author to keep the profession *au courant* with the rapid progress which physiological science is making, by the issuing of repeated editions of his excellent and satisfactory treatise. W. A. H.

ART. XVIII.—*Medico-Chirurgical Transactions.* Published by the Royal Medical and Chirurgical Society of London. Volume the forty-third. London, 1860. 8vo. pp. 397.

THIS volume of *Transactions* contains thirty-five original communications. Ten of these communications, or abstracts of them, were published in the numbers of this Journal for last year, in the quarterly summaries of improvements and discoveries in the medical sciences. The following is an analysis of its contents:—

I. *History of an Additional Case of Tubal Gestation.* By ROBERT LEE, M. D.—The patient in this case was a woman, whose age is not given, who died suddenly from internal hemorrhage, one month after menstruation had taken place as usual. At the autopsy, three pints of blood were found in the sac of the peritoneum, that had escaped from an opening in the left Fallopian tube, where an ovum, containing a perfectly formed embryo, was discovered. Inclosing the chorion everywhere, and adhering to the inner surface of the tube, Dr. Lee found what he calls the *membrana decidua.* An opening existed in this membrane, which corresponded with the opening met with in the Fallopian tube, through which the blood had poured into the peritoneal cavity.

On the occasion of a communication made by Dr. Lee to a former volume of *Transactions*, some reasons, which we deem sufficient, were given for differing with him in regard to the formation of the membrana decidua.[1] Those interested in the subject will also find some important information in the paper of Dr. Hicks, entitled "Remarks on two Cases of Extra-Uterine Fœtation," contained in the last volume of *Guy's Hospital Reports*, a notice of which will appear in the next number of this Journal.

This communication is accompanied by a plate representing the uterus and the right Fallopian tube, containing the ovum, which has been opened.

II. *On the Etiology and Treatment of Peritonitis.* By SAMUEL O. HABERSHON, M. D.—An abstract of this paper is published in this Journal for April, 1860.

It is stated simply in this abstract that, in 56 cases where the peritonitis was caused by perforation of the intestine, 15 were from continued or typhoid fever. It is interesting to know, moreover, that the fatal lesion took place at the lower part of the ilium, and generally about the twenty-first or twenty-second day of the fever. Several perforations were produced, or at least accelerated, by removal to the hospital. Five other cases are mentioned, in connection with fever as a cause of peritonitis, in two of which the perforation was not complete, the peritoneum being left; in a third case, the intestine had given way, but local

[1] See this Journal for July, 1859, p. 178.

abscess was the result rather than general acute peritonitis; in a fourth, chronic adhesions had formed; and in the fifth, described as a case of fever, vomicæ were found in the lungs, and only very slight ulceration in the ilium.

III. *Notes of a Visit to the Leper Hospital at Granada.* By JOHN WEBSTER, M. D.—This communication contains a short but very interesting memoir of a visit to the ancient Leper Hospital at Granada, which hospital was founded by Queen Isabella, about three centuries and a half ago. The establishment, at the time of Dr. Webster's visit, contained 53 inmates, in whom the disease could be witnessed in all its stages; some individuals having but a slight eruption on the skin, while others formed an almost indescribable mass of bodily corruption.

As the result of his investigations, Dr. Webster deduces the following general inferences:—

1st. Leprosy chiefly affects the male sex, as it always has done heretofore.

2d. Every age is liable to its attacks, but mostly that after puberty and during manhood.

3d. The malady is not infectious, in the strict sense of that definition.

4th. It seems to be an endemic disease.

5th. Occurs only among the lower and badly fed ranks of society, at present.

6th. Residents on the sea-coast constitute its ordinary victims in Spain, as elsewhere.

7th. The principal apparent causes seem to be putrid, indigestible, and innutritious aliment; also indigence, filth, and occupying insalubrious dwellings in marshy, miasmatous soils, or where a humid, variable, marine atmosphere prevails.

8th. Leprosy may be communicated by inoculation, according to some authorities.

9th. It is incurable, when fully developed; hence, in the latter stages, all medical treatment proves unavailing.

10th and lastly. Although the disease seems nearly extinct in districts where leprosy formerly prevailed extensively, and has almost disappeared from every other European country, excepting Greece and Norway, it yet still exists to some extent throughout Spain, especially among poverty-stricken natives who live either upon or near the southern Mediterranean sea-shores of that peninsula.

IV. *Observations on Stertor, and on the varying Conditions upon which it is dependent; with the Treatment necessary to its Relief.* By ROBERT L. BOWLES.—An abstract of this communication is published in the number of this Journal for April, 1860. It is a communication of great interest, and at the same time of great practical importance. The influence of position is almost always strictly attended to in every surgical case; while, in medical cases, it is very generally overlooked. When there is paralysis of the muscles of the tongue and fauces, as in apoplexy, or from the action of chloroform, alcohol, etc., this neglect, as Mr. Bowles shows, shortens life, and even, at times, suddenly cuts it short.

A plate is attached to this communication containing two figures, representing the position of the tongue when the mouth is closed or open.

V. *On a Form of Secondary Syphilitic Inoculation.* By HENRY LEE.—In this communication, Mr. Lee gives a number of cases which, he believes, show that secondary syphilitic disease may be communicated, by contact, from one individual to another, and that one form in which it is so communicated bears a very strong resemblance to the primary indurated chancre. The reason why many secondary syphilitic affections are not capable of being communicated is owing, in his opinion, to the fact of their not yielding any fluid secretion.

A plate containing three figures accompanies this communication, showing the appearance of the inoculated sores and of the enlarged glands attending them.

VI. *On a New Method of Operating for the Radical Cure of Hernia.* By JOHN WOOD.—An abstract of this communication is published in the number of this Journal for July, 1860.

All methods of operating hitherto practised for the radical cure of hernia,

have been shown by experience to be more or less dangerous and more or less ineffectual. Of late years, the favourite method has been that of Wützer—whose name, by the way, is invariably written *Wurser* in the present communication. This method, though rarely, if ever, followed by inflammation of the peritoneum—which is the great danger in all these operations—is generally conceded, at the present time, to be entirely unsatisfactory; the rupture re-descending either immediately on leaving off the instrument and assuming the erect posture, or after discontinuing the truss worn after the operation. The chief cause of the failure of Wützer's operation is believed by Mr. Wood to be the inefficiency of the steps taken to cause adhesion of the surfaces of the posterior fold of the invaginated sac together and to the posterior wall of the hernial canal. By the method of operating which he recommends, as is seen in the above-named abstract, the abdominal opening and the inguinal canal are drawn together and compressed by ligatures introduced subcutaneously. In this way, the structures forming the opening through which the hernia protrudes, are agglomerated into a solid mass, not of new and adventitious tissue, but of the more permanent tendinous and fascial tissues.

Mr. Wood states that he has performed his operation in fifteen cases of hernia. In all, except two of them, the general symptoms were so slight as to excite little attention. In one case, there was copious suppuration and burrowing of matter between the muscles of the abdomen, with tympanitis, possibly associated, Mr. Wood admits, with some parietal peritonitis. In one of the cases, there was a return of the hernia three weeks or a month after the operation, from attempting to lift too great a weight *and without a truss.* This return is attributed to the imprudence of the patient, a sailor boy, in omitting to wear a truss. It is strange that this reason should be given by Mr. Wood, for on only the previous page we find it said that trusses with strong springs are most injurious after his operation, by favouring the absorption of the effusion within the canal; that the occasional and intermittent use of a weak instrument is, on the contrary, beneficial, by exciting contraction in the canal; and that the usual effect of a truss worn continuously after an operation for the radical cure, is to mask its failure. It would be generally understood from such statements, that a weak truss occasionally used could do good by its action on the structures about the canal, aiding them to contract; but that a strong, compressing instrument was not only unnecessary after his operation, but, on the contrary, even injurious. In attributing, therefore, the return of the rupture to the fact of no truss having been worn, Mr. Wood would seem to contradict his previous statements.

We confess that we can see nothing in the peculiarities of Mr. Wood's method of operating, or in the results he has attained therefrom, to induce us to change our opinion as to the dangers and the inefficiency of all operative procedures for the radical cure of hernia.

Two plates, containing nine figures, are attached to this communication, by which the understanding of Mr. Wood's peculiar mode of operating—which is certainly one attended with considerable difficulty—is greatly facilitated.

VII. *On the Nature of Ovarian Cysts which contain Teeth, Hair, and Fatty Matters.* By ROBERT LEE, M. D.—This communication, which contains the histories of several very curious cases never before published, and a reference to a number of others to be found scattered here and there in medical writings, is a valuable contribution to the obscure subject of which it treats.

A plate is attached for the purpose of illustrating one of the cases recorded in the paper.

VIII. *Case of Intra-Uterine Fracture, with Observations to show the Analogy between Fractures in Utero and Congenital Distortion.* By BERNARD E. BRODHURST.—A short abstract of this interesting paper is published in the number of this Journal for October, 1860.

The author has evidently been at very considerable pains in searching for cases recorded of intra-uterine fracture. It is, therefore, remarkable that he has entirely overlooked Malgaigne (*Traité des Frac.*, p. 30 *et seq.*), who has devoted several pages of his treatise, with his usual display of erudition and critical acumen, to their consideration. There are also several cases recorded in the

recent work of Dr. Hamilton, and the one related by him at page 38 would be a very valuable addition to Dr. Brodhurst's list. He has himself cited none where the connection between intra-uterine fracture and distortion is so well marked.

IX. *Case of Femoral Hernia which had descended beneath the Pectineal Portion of the Fascia Lata, and was partially covered by the Pectineus Muscle.* By John Adams.—The patient in this case was a woman, 45 years of age, who, after labouring for several days under the ordinary symptoms of strangulated intestine without there being any symptom of hernia, but a fulness considerably to the inner side of the usual site of femoral hernia, was operated upon by Mr. Adams. Cutting down in the direction of this fulness, he found a hernial sac, which contained some fetid pus, a portion of dark omentum, and about an inch and a half of small omentum in a gangrenous condition. The woman died four days after the operation, and, at the autopsy, the hernia was seen to have descended through the femoral ring, not in the sheath of the vessels, but behind and somewhat internal to the femoral vein, making a deep excavation in the pectineus muscle.

X. *Case of Imperforate Arch of the Aorta in which the Root of the Aorta was Ruptured.* By Thomas Alfred Barker, M. D.—This communication gives the history of a very curious case, where, a young man, 24 years of age, after always having enjoyed excellent health, was suddenly attacked, while lacing his boot, with severe pain in the left side of his head, in the left arm, and in the lower half of the anterior part of the chest, accompanied with palpitation and a sense of suffocation. He died suddenly, nineteen days afterwards, and on examining the body the commencement of the aorta was found dilated and ruptured in two places, the ruptures opening into the cardiac cellular tissue subjacent to the pericardium. There was pericarditis, and extravasation of blood into the heart. Some blood was found in the cavity of the pericardium, that had escaped from a rent in its visceral layer, which had probably taken place immediately before death. About half an inch beyond the ductus arteriosus, there was complete congenital constriction of the aorta.

XI. *On the Treatment of Axillary Aneurism.* By James Syme.—An abstract of this communication, together with a just and comprehensive criticism made upon it, by Mr. Erichsen, at the time of its reading before the Society, is published in the number of this Journal for July, 1860.

XII. *Report on the Condition of the Prostate in Old Age, founded on the Dissection of One Hundred Specimens in individuals over sixty years of age.* By John Cockburn Messer, M. D.—A full abstract of this communication, together with the remarks made at the time of its reading before the Society by Mr. Henry Thompson, whose researches, already noticed in this Journal, are remarkably confirmed by those of Dr. Messer, is published in the number of this Journal for July, 1860.

XIII. *On the Condition of the Blood in Mania.* By W. Charles Hood, M. D.—This communication, of which a short abstract was published in the July number of this Journal for the past year, is an important contribution to what is already known of the condition of the blood in various diseases.

XIV. *Cases of Cerebral Affection caused by Disease in the Region of the Nose and Eyes.* By Hermann Weber, M. D.—Cerebral affections caused by disease of the internal ear are not of rare occurrence, while they are very seldom seen to accompany diseases existing in other regions adjacent to the cavity of the cranium. The three cases recorded in this communication will, therefore, be found particularly interesting. In two of them, from the post-mortem examination, it was manifest that the disease within the cranium commenced with coagulation of the blood in the cavernous sinus, in consequence of the introduction of morbid substance from the original seat of the disease in the nose and the eye, through the ophthalmic vein.

XV. *On Congestion of the Heart and its Local Consequences.* By William Jenner, M. D.—The object of this paper is to call attention to the occurrence

of congestion of the muscular tissue of the heart from mechanical impediment to the passage of the blood; to the induration, toughening, and thickening of the walls of the heart caused by this congestion; and to the influence which those changes of texture exercise, as predisposing causes, on the development of permanent dilatation of this organ.

XVI. *Case of Acute Caries of the Walls of the Tympanic Cavity, producing Ulceration of the Internal Carotid Artery.* By JOSEPH TOYNBEE.—The case related in this paper is interesting principally from its rarity; indeed, only one other similar case, so far as Mr. Toynbee's researches have enabled him to discover, has ever been recorded. Both cases terminated fatally, from hemorrhage.

A wood-cut is attached to this communication, representing the condition of the carious bone and the ulcerated internal carotid artery.

XVII. *On Diseases of the Kidney accompanied by Albuminuria, considered in relation to their Origin in change occurring in the Tubes or in the Intertubular Structure.* By W. H. DICKINSON, M. B.—The peculiar views of Mr. Dickinson, in regard to the capability of arranging into two great classes those morbid conditions of the kidney called after the name of Dr. Bright, may be learned from an *extract* published in this Journal in January, 1860, which extract is a good summing up of the views and arguments contained in the present communication.

Three plates, containing five figures, are added to this paper, to illustrate the different conditions of the kidney.

XVIII. *Glaucoma and its Treatment by Iridectomy.* By J. W. HULKE.—A full abstract of this communication is published in the number of this Journal for October, 1860.

XIX. *Contribution to the Study of Spirometry.* By T. GRAHAM BALFOUR, M. D.—This contribution to the study of spirometry is made up from the hospital books and records of the Grenadier Guards, where the height and "vital capacity" of each recruit are given, and where, also, his medical history is to be traced from the time of his joining the regiment. The number of recruits taken was 1126; the average time of service amounted to three years and five months.

The identity of the results of the well-known observations of Mr. Hutchison and those here examined by Dr. Balfour is very remarkable, and may fairly be deemed an evidence of their accuracy.

XX. *Inquiry into the Treatment of Congenital Imperforations of the Rectum by Operation, founded on an Analysis of One Hundred Cases, nine of which occurred in the Practice of the Author.* By T. B. CURLING.—This communication, of which an abstract is published in the number of this Journal for October, 1860, is the longest, and at the same time the most important, paper in the whole volume. Of the many congenital malformations to which the body is liable, there are few more fatal to the life of the infant, or more distressing to the parents, than those here considered. At the same time, until the appearance of the very recent publication of Dr. Bodenhamer, it was very difficult, without considerable research, to acquire information in regard to the operative measures to be undertaken for their relief. We venture to express the hope that the study of this paper of Mr. Curling, as also that of the work of Dr. Bodenhamer, may lead to a change in the doctrines taught by two of the most distinguished teachers of surgery in this country. Dr. Bigelow, of Harvard University, thinks it unnecessary to discuss the question of "eking out the life of a new-born baby by an artificial anus in the groin or back" (*Boston Medical and Surgical Journal*, vol. lvii. p. 512); and Dr. Gross declares that he is "struck with astonishment that any one possessed of the proper feelings of humanity, should seriously advocate a procedure so fraught with danger, and followed, if successful, by such disgusting consequences." (*A System of Surgery*, vol. ii. p. 766.) A further acquaintance with the subject, such as is now afforded to the profession by these recent publications, will show the propriety and safety of teaching doctrines more in consonance with the spirit of our profession and of our civilization.

XXI. *On the Relative Amounts of Sugar and Urea in the Urine in Diabetes Mellitus.* By SYDNEY RINGER.—An abstract of this communication is published in the number of this Journal for October, 1860.

XXII. *Tabular Statement of Seventy-two Cases of Hæmatemesis; with Remarks.* By C. HANDFIELD JONES, M. B.—Of these 72 cases of hæmatemesis collected by Mr. Jones, 53 were females, 19 males—a proportion of nearly 3 to 1.

The hemorrhage occurred before the age of 20 in 2 males and 3 females; from the age of 20 to 40 in 9 males and 36 females; after the age of 40 in 8 males and 14 females.

In 28 cases, indigestion is mentioned as occurring a month or longer prior to the hæmatemesis. In 12 cases, it is stated that indigestion was absent or very slight.

In 40 cases, the existence of gastric ulceration seems to be more or less certain. In 32 cases, its existence is more or less doubtful.

In 3 cases, the liver was in a state of cirrhosis, and in 1 in a state of acute yellow atrophy; in 1, the spleen was greatly enlarged and there was ascites; in 2, the presence of cancer in the stomach was highly probable; and in 5 cases, hæmatemesis had a distinct relation to the catamenial periods.

XXIII. *Account of a Fresh Dissection of a Preparation of Tubal Gestation, described by Dr. John Clarke, in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge for* 1793. By T. HOLMES.—The chief point of interest in this communication is the determination, by a careful dissection, of the existence of a stout membrane, of a fibrous appearance, surrounding the chorion in the whole of its extent.

A plate is attached to this communication, representing the appearance of the uterus and Fallopian tube.

XXIV. *Cases of Obstruction of the Veins of the Lower Extremities causing Œdema of the Corresponding Limb, and occurring in Phthisical Patients.* By GEORGE CURSHAM, M. D.—The cases of venous obstruction related by Dr. Cursham are three in number, and he refers to several others that have been recorded by various authors. He believes the coagulation in the vein to be produced by the presence of pus in the blood. All the cases in which it was witnessed were in the last stages of phthisis, the lungs containing numerous suppurating surfaces, from which pus or some foreign matter might readily be conveyed into the circulation.

XXV. *Case of Recurrent Anæsthesia of almost the entire Surface of the Body, accompanied by partial Loss of Motor Power in all the Limbs, traceable to the effects upon the Spinal Nerves of Effused Products within the Spinal Canal.* By JOHN W. OGLE, M. D.—This case is one of peculiar interest on account of the close and intelligible dependence of the symptoms upon the lesion, as determined after death. The comprehensive title of the communication will serve to give a correct idea of the nature of the case. W. F. A.

ART. XIX.—*Transactions of the Epidemiological Society of London.* Vol. I., Part I. 8vo., pp. 136. London, 1860.

THE Epidemiological Society of London was organized in the Summer of 1850. Its chief object is, as its name imports, "the investigation of epidemical diseases;—the institution of a rigid examination into the causes and conditions which influence their origin, propagation, mitigation, prevention, and treatment;—the collection and promulgation, with relation to these subjects, of such facts as appear to be established on sound and sufficient evidence, and the indication of those methods of investigation by which the misleading influence of false or deficient facts may be avoided.

The society embraces among its members a large number of physicians and others interested in the leading object for which the society was instituted, and who, from their respective positions—their habits of careful observation, close investigation, and cautious induction, are well qualified to contribute valuable materials towards carrying out that object to a successful accomplishment.

Through the medical periodical press of Great Britain we have been made acquainted with the results of the first nine years of the Society's operations. Convinced that it is only by the publicity given to its transactions, that its efforts can be rendered generally useful—that the promotion through it of the public good can only be accomplished by imparting to those beyond the comparatively narrow circle of its members the facts which these have elicited, and the general conclusions to which they have been led by their observations—thus adding them promptly to the common stock of professional knowledge,—abstracts of papers read at its monthly meetings were furnished to the medical journals. It being found, however, that such abstracts furnished, at best, but a general idea of the materials elicited by the operation of the Society, and that the promotion of the good of the community and the enlightenment of the medical profession on subjects of the deepest interest as matters of scientific investigation, and of the utmost importance in their directly practical application, would be best and most certainly attained by the publication from time to time, as materials accumulated, of their transactions in a separate form, and under the immediate superintendence of a suitable committee; the Society accordingly determined upon such a course, and, as its first fruit, we have the volume which lies before us.

It opens with the annual address, delivered Nov. 7, 1859, of the President, Dr. Benjamin G. Babbington. The address presents a very neat but brief *resumé* of the transactions of the Society during its ninth session—with a general sketch of the principal epidemics which had occurred during the preceding twelve months in Great Britain, the British colonies, the United States, and other lands. The address is replete with interest—it is not one, however, adapted for analysis, while it presents no prominent points demanding especial comment.

The first of the scientific papers is on "the Theory of Zymosis," by Dr. B. W. Richardson. The author denies that the virus of diseases of the zymotic class has in the proper sense of the term any reproductive property or power, when conditions favourable to such reproduction are supplied. The view which he presents as explanatory of the propagation of all communicable diseases is, that "there is a step in the process, which consists in the development of an albuminous matter having the power of exciting chemical changes. or zymosis in the organism; and that there is a second process, consisting of a modification in the animal chemistry, in the formation, that is to say, of new and inorganic poisons, upon the presence of which the symptoms of the disease depend."

Dr. R. believes that in the production of communicable and some other diseases there really takes place such a process as zymosis, or fermentation, in the animal body. The views he offers in respect to such process are:—

"1. That the zymosis, whether occurring in the body or out of it, though manifested always by the agency of organic matter, is essentially a chemical process, leading to the resolution of complex organic compounds into simpler compounds. 2. That when the process occurs in the body, the new chemical products are the causes of the symptoms as well as of the pathology of the diseases excited. 3. That the virus of these diseases is purely incidental to a modified chemistry in the organism; is excrementitious; and is as necessarily an excrete under the modified conditions, as urea is necessarily an excrete in perfect health. That this excrete, as an albuminous product, possesses the power of exciting the zymosis in a person liable to such action, and is connected with symptoms, not by an immediate organic increase of its own parts, but by the establishment of a new series of chemical changes."

This view of zymosis Dr. R. believes will explain satisfactorily many points which, otherwise, are all confusion, and at the same time link certain diseases to the zymotic not now ranked on that list, while it will reduce the verbal list of the zymotic to a few units. It shows why diseases directly communicable

from person to person resemble other diseases which have a tendency to spread, but without being communicable. Thus, he remarks:—

"The poisons of phytogenic fever and of dissecting-room diarrhœa are not communicable poisons in the strict sense of the word. Yet they give origin to particular cases of disease which may be considered endemic, and by extension of cause, epidemic.

"A second explanation supplied by this theory relates to the quantity of poison required for the production of symptoms. It has often been a subject of wonder why so minute a portion of the virus of a disease, such as smallpox, excites such decided effects. Dr. Fordyce largely diluted the virus of the above named disease, hoping thereby to induce a milder form of the disorder by inoculation. He was mistaken in his hopes; for he found that the disease, if produced at all, was as severe, after extreme dilution of the virus as when the virus was directly used. The reason for this is sufficiently simple when we recall that, in zymosis out of the body, the external conditions and supply of matter susceptible of zymotic change being supplied, the quantity of ferment neither removes the fact of fermentation, nor modifies the process."

The theory of zymosis advanced by him, according to Dr. R., suggests, also, an alliance between diseases arising from the absorption of an external poison and diseases which are produced by changes occurring in the body, independently of an exterior agency which could, with propriety, be designated as of a poisonous nature.

"In this sense," he remarks, "acute rheumatism is a disease connected in a degree with what are called the zymotici. This disease in all its symptomatic phases, is due to the presence of a poison in the blood. We now, indeed, know the nature of this poison; we have an idea of the part of the system in which it is produced; and we are conversant of the mode by which it is eliminated. Now, if acute rheumatism could be excited by the inoculation into a healthy body of a virus which would start the formation in the blood of the acid poison on which the fibrous inflammation depends, then rheumatism would be palpably a zymotic disease. As the disease cannot be so produced directly, it has not been considered a zymotic, notwithstanding that its cause is a poison generated in the animal laboratory, and that it is itself one of the purest representatives of the zymotic class."

Tetanus is another disease Dr. R. considers to belong to the zymotici.

"In this disease, the poison, in my opinion," he writes, "is first developed in the wound, as a result of decomposition. Thence carried into the circulation, the new substance, without any necessary increase of its own parts, excites a zymosis, ending in the production of an alkaloid or alkaline body which has all the power of exciting the symptoms of spasm—as much the power as strychnine itself. Nay, in regard to this tetanic poison, we have, as I have pointed out in a previous paper, a clue to the nature of the poison. I showed then that all the specific tetanic symptoms could be induced by the introduction into the body of certain of that series of the ammonias into which carbon enters as a constituent element.

"In the next place, the theory propounded explains the reason for the fact why the majority of epidemic diseases are not fatal, if the patients in whom they occur are placed in favourable conditions for recovery. We have seen that, if the hypothesis of the constant reproduction of an organic poison in the body, and of the presence of this poison as the cause of the symptoms, were tenable, the process of reproduction of the poison should continue so long as the patient could supply the means for the continuance of the process, that is to say, until death itself. When, however, we take the chemical view of the question, when we consider that the organic virus primarily introduced is not immediately propagated, but induces chemical changes upon which the symptoms of the disease depend, we see a reason why the epidemics run a limited course; since chemical changes, having no independent continuance when their causes are removed, necessarily cease after a time, together with the symptoms which have been excited by them during the period they were in operation.

"If," Dr. R. remarks, in conclusion, "there is any truth in the theory of zymosis in the body, its principles or laws are at hand, and are limited. All the

primary products of fermentation reduce themselves into two groups, the acid and the alkaline, or the alkaloidal. Experiment demonstrates an analogy, as existent between the action on the body, of certain poisons which would result from zymosis externally produced, and the symptoms of some zymotic diseases developed spontaneously, in the which these very same products are generated."

The views advanced by Dr. R., in explanation of the production of zymotic diseases are deserving of a careful study. They are, to say the least of them, extremely plausible.

The next paper is "the Report of the Diphtheria Committee." The entire document with its appendices is one of deep interest. The disease to which the facts it embraces refer, notwithstanding it has prevailed extensively in former years, and been accurately described by several prominent writers, cannot be said to have been studied with the closeness and care required to throw full light upon its etiology, nature, and treatment. It is to the opportunity afforded by the prevalence of the malady in Europe and this country, that we are to look for a more careful investigation of every particular having a direct bearing upon its pathology and therapeutics; nor has the opportunity been neglected. Among the many contributions to which the investigation of the present epidemic has given birth, the one before us will hold a very prominent rank, as well for the value of the materials it furnishes as for the caution which appears to have been observed in their collection and their record.

We can only command space for the general summary of the facts of chief importance embraced in the report, which are as follows:—

"1. The occurrence of sporadic diphtheria in the county of Kent, in the years 1851–52 and 54, and probably also in 1855–56, and in the county of Essex, in the year 1854.

"2. The probable occurrence of sporadic diphtheria in different parts of the kingdom, at intervals within the last twenty years.

"3. The unusual prevalence of throat affections of all kinds, and not unfrequently of a peculiar kind (apparently, if we may so phrase it, of a *quasi*-diphtheric character), prior to or contemporaneously with the present epidemic of diphtheria.

"4. The concurrent or intercurrent prevalence in the same locality of epidemic diphtheria with epidemic scarlet fever, and the occasional occurrence of a diphtheritic affection of the fauces in scarlet fever.

"The whole of these facts have an important bearing upon the history of the development of the present epidemic of diphtheria—(*a*). In reference to the introduction of the disease into the kingdom from without: (*b*). In reference to the causes which have led to the development of the epidemic: (*c*). In reference to the relations of the disease to cognate affections and scarlet fever. Upon all these points the foregoing facts are valuable as suggestive of further inquiry, and as indicating the direction in which that inquiry should tend, as well in those districts in which the epidemic has prevailed, as in those in which it now prevails.

"5. The doubtful contagiousness of the disease in some districts, its *undoubted* contagiousness in others.

"6. The unsanitary state of the majority of the localities in which the disease occurred.

"7. The greater liability to the disease being in the first decennium, or, to narrow the question, the second quinquennium of life.

"8. The proportion of *males* in one hundred cases was 42.3; of *females*, 58.2.

"9. The nature of the treatment indicated: (*a.*) The application of a more or less stimulating or caustic substances to the diseased parts; (*b.*) The regulation of excretions; (*c.*) The sustentation of the powers of the system."

To the foregoing we would add that, in the report, the duration of the disease is specified in *thirty-three* fatal cases, namely—

In 2 it continued 3 days.	In 4 it continued 7 days.	In 1 it continued 13 days.
" 2 " 4 "	" 1 " 9 "	" 3 " 14 "
" 5 " 5 "	" 4 " 10 "	" 1 " 17 "
" 3 " 6 "	" 3 " 11 "	" 1 " 18 "
	3 " 12 "	

The mortality varied in different localities from 0 to 100 per cent. The average of the cases reported to the Committee—514 cases, 96 deaths—being 18 per cent.

The mode of death is specified in 22 cases only. *Twelve* of these died from *exhaustion; one* from *hepatitis* on the third day; *three* from *laryngitis* (one on the second, and one on the first day); *five* from *suffocation*, and *one* from *asthenic pneumonia*.

No post-mortem examinations were made.

The paper which follows contains some pertinent suggestions for utilizing the statistics of disease, as it occurs among the poor; for so employing them as to enable us to derive from them valuable and accurate data for determining the most frequent disabling diseases among the labouring classes in the towns, villages, and rural districts; the prevalence and severity of these diseases in different districts and localities, and the influence of age, sex, condition, modes of life and occupation, in their production; particulars, a knowledge of which is essential to a correct appreciation and right estimate of the principal predisponent and exciting causes of those diseases, with a view to their abatement or prevention.

Believing it fair to presume, remarks Dr. Gavin Milroy, to whom we are indebted for the suggestions in question, that the plan adopted in one department of the public service with success, is equally applicable to another, he points to the salutary changes already effected in the sanitary condition of the army and navy of Great Britain, based upon reforms, the suggestion of which were derived from the statistical returns of the naval and military medical officers. Five and twenty years ago no use was made, at least to any extent, of the constantly-accumulating experience of the medical staff of either branch of the service, in reference to the average annual amount of sickness, mortality, and disablement among the soldiers and sailors. It had been long surmised that, at all times, and in all climates, this amount was high—higher a good deal than it was thought it should be. It was known, too, that every now and then the losses from epidemic disease were excessive in barracks, and on board ships of war. Still, no measures for the general improvement of the health of the men could be satisfactorily devised until all the facts bearing on the subject, and contained in the returns and reports of the medical officers were examined and sifted. This was at length done, and the work has since been systematically carried out; and with the best results in the way of making known the defective arrangements which told injuriously upon health, and of leading to various improvements, which have had the effect of materially reducing the proportion of sickness and mortality among the men.

Now, Dr. Milroy is persuaded that were something of the same sort done with the mass of instructive details contained in the returns and reports of the parochial medical officers, equally beneficial results, in reference to the sanitary condition of the poor, would ensue; more slowly and gradually, it is true, but in the long run not less decisively. The poor and labouring classes suffer everywhere a disproportionate amount of sickness and death, from circumstances not inevitable or inseparable from mere poverty, or from their respective locations and employments, but which are superadded to those from either ignorance or neglect, or from causes over which they have themselves no control, however capable the evils may be for correction or removal.

An annual report, founded upon an examination of these documents, and embodying their leading facts and general results, could not fail, it is believed, in conjunction with the admirable yearly reports of the Registrar-General, to prove of the highest value to the science of public hygiene and topographical medicine.

The remaining paper in the present volume of Transactions, presents a series of remarks on the topography and diseases of the Gold Coast—the west coast of Africa. It is by R. Clarke, Esq., late surgeon to the natives. It is full of curious and interesting details in respect to the condition, habits, customs, and occupations of the natives, topography and meteorology of the country, the prevailing diseases, and the general influence upon the health of the climate. The entire paper will repay a careful perusal.

We cannot command room for even a brief notice of either of the subjects embraced in it.

Dr. C. would attribute much of the reputed insalubrity of the Gold Coast to the neglect on the part of the Europeans who visit it, of the necessary precautions required for the preservation of health. They frequently fall into excesses which the best constituted could not bear with impunity in England. Were the requisite sanitary laws obeyed the climate would prove, he believes, quite as favourable to life and health, or even more so, than others in the same latitude. Among the most important of the means of preserving health to be observed by Europeans visiting the coast, are temperance, regular and industrious habits, appropriate clothing, and daily exercise in the open air. Many entertain an impression that the sun's rays are hurtful, whereas, in nine cases out of ten, we are assured, that the mischief is not done by the sun's rays, but by errors of "personal economy."

Dr. C. informs us that among the classes best adapted to resist the climatorial influences of West Africa, the negro race holds the first rank; still, he thinks it questionable whether they enjoy this privilege to a much greater extent than persons of pure European blood, provided the latter are sober, cleanly, and active in their habits. It is true, that upon their first arrival in the country Europeans run a greater risk of incurring fatal disease than the blacks. When once they have become acclimated, however, they seem, according to Dr. C., generally to withstand the influence of the climate better than the latter; provided, always, they are temperate in their habits. D. F. C.

ART. XX.—*The Composition of the Urine in Health and Disease, and under the Action of Remedies.* By EDMUND A. PARKES, M. D., Fellow of the Royal College of Physicians, Professor of Hygiene in the Army Medical School, &c. &c. London: John Churchill, 1860. 8vo. pp. 404.

IF there be any one subject in medical science which is pre-eminently a favourite with original observers and investigators, it certainly is the urine. In every journal, and in the proceedings of every medical society, we are sure to find some recent contribution to urology, whilst monographs upon the subject are being sent out with a rapid order of succession, arguing the most unbounded faith on the part of publishers, and which is only equalled by the perseverance of the authors; and yet, notwithstanding the number of treatises on the urine, there are few, if any, which do not contain something of interest, and which are not marked by some distinguishing peculiarity. The subject is so vast, and can be treated of from so many points of view, that each separate work is sure to exhibit some feature of importance not met with in any of its predecessors; add to this, the fact, that the urine contains a great part of the ashes of the effete tissues, and the additional fact that it admits of easy examination and analysis, and we have two of the main reasons which have attracted physiologists to the study of this important excretion.

The work before us is no exception to the statement that every treatise upon the subject to which it relates, is marked by some important peculiarity. In this case the feature is that of presenting a complete record of all the observations which have been made by physiologists upon the composition of the urine in health and disease, showing the various alterations which ensue under certain fixed conditions in the proportions of the several substances which go to make up the excretion in question. The following extract from the preface sufficiently indicates the scope of Dr. Parkes' treatise.

"I have attempted in this work to give an abstract (necessarily extremely condensed, but I hope accurate), of the very numerous observations on the 'Urine of Man' which have been made of late years. It seemed to me desirable to compress, if possible, into some narrow compass, the records of researches of great interest, but yet so elaborate in their details, and so scattered in different

publications as to be almost inaccessible to men engaged in active professional work. To anything like a full account, I cannot lay claim, but I hope I have succeeded in presenting a readable book for those who wish merely to know the latest observations on this subject, and in furnishing to those engaged in similar original inquiries, some assistance in the way of references. I have given the references carefully, not from any display of pedantry, but because I am quite convinced that, in the present state of this difficult inquiry, every statement should be easily traced to its author, so as, if necessary, to be properly verified.

"The title of this work expresses its exact nature; it is a mere enumeration of the alterations in the urinary constituents, under various circumstances. I have not entered into the chemical history of these constituents, nor into the mode of determining their amounts. Works on chemistry deal rather with the first subject, and the technical treatises of Neubauer and Thudichum have, for a time, exhausted the second."

We do not intend to give a full analysis of Dr. Parkes' volume, in fact, such a work would be almost impossible, neither would a critical review of his labours be necessary, after having expressed the opinion we have upon the subject. The work, however, affords us an opportunity of laying before our readers an abstract of some of the more important observations which Dr. Parkes has collected relative to the amount of urine excreted by the human organism, and the proportions of the several constituents of that fluid.

Quantity of Urine.—Twenty-three observations are referred to by Dr. Parkes, showing a range from 35 to 67 ounces in the twenty-four hours, about 52 ounces being the average. It is seen, therefore, that the quantity of urine is subject to very considerable variation within the limits of health, being affected by the amount of fluid imbibed, the temperature of the atmosphere, &c.

Urea.—This substance is also excreted in variable amount, ranging from 286 to 688 grains in the twenty-four hours, the mean being 512 grains. Several of the lower amounts were obtained by processes which were not capable of giving accurate results. Food, physical and mental exercise, are the main factors which influence the quantity of this substance excreted.

Creatin and Creatinine.—The amount of these substances has not been definitely determined. Thudichum is the only observer quoted. According to his investigations, on two individuals, each 28 years old, the first of these constituents amounted to 4.7 grains, and the second to 7 grains. They are generally estimated as *extractives*.

Urine Pigment.—Nothing is known relative to the amount of this substance excreted by the kidneys.

Extractives.—"Under this head are included various substances, viz., pigment, creatin, creatinine, xanthin, hypoxanthin, sarcine (?), xanthroglobulin, the resinous substances called omichmyloxide by Scharling; the resin discovered by Harley, which adheres so strongly to the pigment (perhaps the xanthin of Strahl and Lieberkühn); the acids discovered by Marcet; the substances containing sulphur and phosphorus, examined by Ronalds; and perhaps a little sugar (?)."

Nothing very certain is known relative to the amount of extractives. About 154 grains in the twenty-four hours, is perhaps a fair average.

Mucus.—Böcker's observations show this to be about 7 grains per day.

Uric Acid.—This constituent ranges from 4.32 to 14.19 grains, in twenty-four hours, the mean amounting to 8.569 grains. It is subject to variation from the character of the food, exercise, &c.

Hippuric Acid.—This substance, from Weismann's researches, would appear to be excreted in much larger quantity than the uric acid, 34½ grains being eliminated when he lived upon a mixed diet, and 17 grains when on animal food.

Oxalic Acid, Formic Acid, and Carbonic Acid are found in the urine, the two former in small amount; the latter in considerable quantity.

Sulphuric Acid.—This constituent varies, according to the observations cited, from 17.34 to 41.14 grains per day, the mean being 31.11 grains.

Phosphoric Acid.—This acid ranges in quantity from 24.70 to 79.07 grains; the average amount in twenty-four hours being 48.80 grains.

Chlorine.—This substance is excreted in amounts varying from 51.87 to

173.23 grains, in twenty-four hours, 126.76 grains being the average of the results obtained by fifteen observers.

Lactic Acid, if present, is so in very small quantity. *Ammonia* is met with, according to Neubauer, in quantity equal to 7 or 8 grains.

Potash, *Soda*, *Lime*, and *Magnesia*, are excreted to the amount of about 120 grains in the twenty-four hours.

The variations which are induced by age, sex, food, drinks, exercise, &c., are next fully considered, and much important information is collected under this head.

The effects of certain medicines are next brought under notice, and then the second book is reached, which treats of the *Urine in Disease*. More than half the treatise is embraced in this section, and little of any importance seems to have escaped the author.

In conclusion, we can recommend Dr. Parkes' work to practising physicians, as one calculated to be of much service to them, and to those commencing or engaged in the study of the physiology or pathology of the urine, it cannot fail to be of great value. W. A. H.

ART. XXI. — *On Diphtheria.* By EDWARD HEADLAM GREENHOW, M. D. Fellow of the Royal College of Physicians, Physician to the Western General Dispensary, and Lecturer on Public Health at St. Thomas's Hospital. 8vo. pp. 160. New York, 1861: Baillière Brothers.

Diphtheria: its Nature and Treatment, with an Account of the History of its Prevalence in Various Countries. By DANIEL DENISON SLADE, M. D., of Boston, Mass. The Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860. 8vo. pp. 60. Philadelphia, 1861.

Diphtheria: its History, Nature, and Treatment. By WM. HENRY THAYER, M. D., Professor of Theory and Practice of Medicine, in the Berkshire, Mass. Medical Institution. From the Berkshire Medical Journal. 8vo. pp. 38 Pittsfield, Mass. 1861.

THE three publications the titles of which are given above have been called forth in consequence of the somewhat extensive prevalence of diphtheria for some time past in different portions of Europe and this country. The first is based upon a careful study of the disease in public and private practice, in conjunction with information obtained in the course of an inquiry made by its author for the English Privy Council in the spring of 1859. It forms a tolerably complete monograph on diphtheria, presenting its history as it prevailed epidemically under different names, in various countries and localities, during the sixteenth, seventeenth, eighteenth, and nineteenth centuries; with copious references to the notices which the disease has received from medical writers who flourished near or during the periods of its prevalence.

The second essay obtained, as stated in its title page, the prize of the Fiske fund for 1860. It presents a very complete exposition of the literature of diphtheria, from the earliest periods to which published notices of the disease can be traced down to the present day, and exhibits a clear and faithful account of nearly all that is known in respect to its symptoms, nature, course, and treatment. The essay will be found entire in the last number, page 145 *et seq.* of this Journal.

The third essay, though less full in its reference to the writers who have treated of diphtheria, presents a very fair and instructive digest of the leading facts that have been developed in respect to its etiology, symptomatology and treatment.

Though comparatively rare as a sporadic affection, especially in its more severe and malignant forms, diphtheria has been repeatedly observed as an epidemic, extending either simultaneously or successively over extensive regions of country, or even distant lands, including portions of the old and new worlds.

More commonly, however, we find it, in certain directions and at particular periods, limited to a single district, neighbourhood, or even house, not a case of the disease even in its mildest form being met with beyond these infected spots.

The entire history of diphtheria, its wide extension at one time, and its limitation within a more or less circumscribed area at another—its tendency to linger in particular localities, or to return repeatedly to the same neighbourhood—its varying intensity in different places, showing itself in one as an affection of great mildness, and in another as one of extreme malignancy and but little under the control of any system of remedial treatment, would seem to indicate that, while the disease owes its origin to some wide-spread aërial influence, some zymotic cause existing in the atmosphere generally, which when received into the blood brings it more or less quickly into a morbific condition, this influence or cause, whatever may be its nature and origin, is modified in the direction and intensity of its action from certain local conditions connected with either population or locality.

Although new to the physicians of the present day in its epidemic form, diphtheria has nevertheless been accurately described under other names by many of the older medical writers. One of the most graphic accounts we have of it is that given by Dr. Samuel Bard, who in 1789 published an inquiry into the nature, cause and cure of the *angina sufficativa* or sore-throat distemper which prevailed in New York in 1771; an affection evidently identical in all respects with the prevailing epidemic.

From the fact that diphtheria and scarlet fever have been very generally observed to prevail simultaneously in the same place, neighbourhood, and house, often even in the same individual, the identity of the two diseases has been inferred by some writers. That diphtheria and scarlatina are closely related to each other there can be little doubt, but that they are not one and the same affection, nor yet, as others have supposed, merely different forms or varieties of the same disease, must, we think, be evident to any one who will carefully study their history. The distinctness of the two would seem to be very clearly shown by the infrequency of a second attack of scarlet fever, and when it does occur its usually mild character, while on the contrary nothing is more common than for the same individual to experience a second or even third attack of diphtheria, within a short period of each other, and often of increased severity. Their non-identity is further shown by the entire failure of any immunity from the attack of scarlatina, in the otherwise unprotected, being afforded by the occurrence of diphtheria, or from an attack of the latter by a previous attack of scarlet fever. Further evidence of their non-identity is not wanting. Thus Dr. Greenhow refers to the absence of any *scarlatinal rash* in cases of pure diphtheria or in connection with such cases during extensive epidemics of the last named disease.

"Albuminuria," Dr. G. remarks, "an occasional symptom of both diseases, occurs at very different stages in the two; in scarlet fever it rarely or never occurs until the primary and characteristic stage of the disease is past. In a series of cases of scarlet fever, in which the urine was carefully examined day by day by himself, with a special reference to this question, he found albumen during convalescence in nearly all, but as early as the tenth day from the appearance of the rash, in only a single instance; more frequently it was not met with until the twelfth or thirteenth day. In only one of the cases, that of an adult, was the albuminuria attended by anasarca. In diphtheria, on the contrary, albuminuria is of less frequent occurrence, if the above-mentioned cases are to be received as an index of the frequency of its existence after scarlet fever, and when it does occur, it is at an earlier period of the disease, sometimes within a few hours of the commencement of the illness. Dr. G. has notes of a case in which he found albumen in the urine thirty-six hours after the first symptoms of indisposition, and before the exudation had attained its maximum intensity."

In addition to the foregoing marks of distinction between scarlatina and diphtheria, Dr. G. notices the frequency of croupal symptoms in the latter compared with the infrequency of their occurence in the former disease. Anasarca, a common sequela of scarlatina, is very rare in diphtheria, as is also the

peculiar kind of arthritis which sometimes follows scarlatina. The same remark may be made with respect to suppuration of the glands of the neck, pericarditis and chorea.

"Besides the absence after diphtheria of the well known sequelæ of scarlet fever, the former disease is succeeded by sequelæ of a character peculiar to itself, and such as have not been found to follow scarlet fever. These are, partial paralysis of the muscles of deglutition and voice, impairment or disorder of vision, paraplegia, hemiplegia, partial paralysis of the upper extremities, numbness of the hands or feet, tenderness, pricking or tingling of the extremities, and gastrodynia. Then, lastly, the occurrence of diphtheria on other parts of the body, as on abrasions of the skin, or wounds, or on the pudenda, has no parallel in scarlet fever. When to these differences we add that the anæmia which soon occurs, and for a long time succeeds to diphtheria, is more intense than in almost any other acute disease, there can be little hesitation in accepting the conclusion that diphtheria and scarlet fever are not the same disease."

A much closer relationship may be traced between diphtheria and epidemic erysipelas, than between it and scarlatina. The two have been known repeatedly to prevail in conjunction. Erysipelas of a severe and unmanageable character will sometimes commence, increase, and decline simultaneously with diphtheria. Dr. Greenhow reports an instance of this commingling of the two diseases as observed by Dr. Nicholson in Reddich, England.

In the monograph of Dr. Greenhow reference is made to the supposed intimate relationship between diphtheria and the lung distemper, or pleuropneumonia of cattle. The two have, in many districts of country, prevailed at one time, and it is a somewhat curious and significant fact that after an absence of many years, within a short period the two should have appeared again simultaneously.

"What renders these cattle epidemics peculiarly interesting in connection with the present subject, is," remarks Dr. G., "the fact that, although the time of their appearance, twenty years ago, they were quite new to the existing generation of dairymen, farmers, and cow doctors (there being no record of their existence in this country during at least the preceding half century), pulmonary murrain, preceded by an eruptive murrain, prevailed about the middle of the last century, just before the outbreak of diphtheria which then occurred. Towards the close of the first half of the century, when Fothergill, Cotton, Huxham, Starr, and other writers, were describing the diphtheria epidemic then prevalent, an anonymous member of the College of Physicians, and Drs. Brocklesly, Hurd, and Layard, wrote their accounts of the murrain then prevailing epidemically among horned cattle. This may, indeed, have been a mere coincidence, but that the appearance of the eruptive and pulmonary diseases among cattle, and of diphtheria in the human subject, are in some measure attributable to the operation of a common cause, seems more than probable, seeing that several of the older writers on *morbus strangulatorius* (diphtheria,) mention its coincidence with certain diseases among cattle. Thus Ghizi says, there was a great resemblance between the epidemic angina which prevailed at Cremona in the years 1747 and 1748, and a disease affecting the respiratory passages at that time prevalent among oxen. Dr. Wall, speaking of the epidemics in England about the middle of the eighteenth century, says, 'This disease has so great a resemblance to the epidemic sickness amongst cattle, that I am persuaded it is of the same nature.' Severinus, who wrote in the seventeenth century, also mentions that a great mortality among cattle preceded the appearance of malignant sore throat, and M. Malouin, in his account of the epidemic diseases observed at Paris in 1746, says, that the disease among cows had already appeared in France when children were attacked by epidemic sore throat. The same reporter, in his remarks upon the diseases of October, 1748, when the form of epidemic sore throat—*maux de gorge gangréneux*—was again prevalent, says it had been noticed that oysters disagreed with every one who ate them during the month, especially before the weather became cold."

Diphtheria has not made its appearance exclusively in any particular season, climate, or locality. The warmest and the coldest season of the year, the most inclement as well as the most pleasant climate; the highest and driest situations

equally with the low and marshy have been visited by the disease. It has appeared in the open country and in the most densely populated cities; in the badly located quarters, and the ill constructed and badly kept habitations of the poor, and in the comfortable well ventilated mansions of the wealthy. In short, the disease has not been found to spare places and neighbourhoods and residences whose sanitary condition is to all appearance unexceptionable, and yet it is evident from the history of the several epidemics of diphtheria on record, and the facts collected by contemporary observers, that it is in those districts, localities and houses which are generally recognized as unhealthy that the largest number of its victims are always found. Here too it presents itself in its worst forms, and is productive of the largest amount of mortality.

The major portion of those attacked by diphtheria have been children between two and twelve or thirteen years of age. The susceptibility to the disease diminishes rapidly after puberty, and is very slight in infancy. The tendency to a fatal result diminishes also with the progress of life. Persons, however, of fifty, sixty, and even seventy years of age have been known to suffer from very severe attacks of diphtheria.

The propagation of the disease by contagion is much insisted on by many writers, while others are as positive that it is neither contagious nor infectious. As is the case with all epidemic maladies, when diphtheria makes its appearance in one member of a household it is very common for most or all the other members to suffer from it either within a very short period of the first, or one after the other at irregular intervals; or, when it occurs in a neighbourhood, to spread as it were from house to house; not because of the contagious character of the disease, however, but from the fact that all who are attacked in the household or neighbourhood, have been exposed alike to one common atmospheric morbific influence. The successive occurrence of the disease in persons all equally exposed to the same epidemic cause can be readily explained without the necessity of supposing that the subsequent cases were produced by a contagious matter emanating from the bodies of those first attacked. It is known that, in very many instances only a single member of a family, and only one or two patients in the same neighbourhood, have suffered from the disease, notwithstanding not the slightest restraint had been interposed upon the free intercourse of the sick and the well. In the report of the committee on diphtheria of the Epidemiological Society of London, the disease is said to have been of doubtful contagiousness in some districts, but undoubtedly contagious in others. A statement which in our estimation casts a very great degree of doubt upon its communicability by a specific contagion in any district.

A digest of the leading facts bearing upon the question of the contagiousness of diphtheria, will be found very fully and candidly set forth in all three of the works before us.

According to Dr. Greenhow, death in diphtheria is in one class of cases preceded by all the symptoms of collapse occurring in cases of fatal gangrene, or by a profuse hemorrhage. Usually death is caused by entire exhaustion of the powers of life by the violence or very protracted course of the disease, or the fatal event is preceded by syncope. In other instances death results from the extension of the diphtheric deposit from the throat into the larynx, presenting thus a mechanical obstruction to respiration. The sudden deaths attributed to exhaustion of the powers of life, there is reason to believe result, in some instances at least, from the formation of heart clots. This is especially the case in those instances of sudden death that occur in patients who had presented no alarming symptoms up to the period of their demise, or at a period of the disease when the patient was, to all appearance, out of danger. The fatal result is in a few instances preceded by convulsions.

Dr. Greenhow enumerates as signs of great danger, the occurrence of croupy symptoms, or of pneumonia, a brown or blackish appearance of the diphtheritic deposit, hemorrhage from the nose, throat, lungs, or intestines; purpura; copious discharge of matter from the nostrils; intense albuminuria; extensive swelling of the glands of the neck; marked diminution of temperature; sickness and diarrhœa, especially at an advanced period of the attack. Either of

the symptoms denotes a severe case; but when, as sometimes happens, two or three are combined, the danger must be regarded as imminent.

Diphtheria is liable to be complicated or followed by bronchitis, less frequently by pneumonia. In the former, besides the stethoscopic signs of bronchial inflammation, there is expectoration of a frothy or glairy fluid, intermingled with which there are often seen casts of the bronchial tubes. The presence of pneumonia, by which diphtheria is occasionally complicated, is frequently unsuspected during life, its existence being first revealed only upon an examination of the body after death. In respect to the morbid anatomy of diphtheria, with the exception of what relates to the abnormal condition of the throat, we have few very accurate observations. In the greater part of the published results of examinations after death in cases of diphtheria, no attempt has been made to distinguish those lesions present which are to be esteemed proper to the disease, and those which had been the result of accidental or intercurrent maladies.

The fauces are generally found after death to be covered to a greater or less extent with an abnormal deposit, varying in consistency from that of a semifluid, pultaceous matter, to that of a firm, consistent, and more or less elastic membrane, having a thickness of from a quarter of a line to several lines. Its outer surface is sometimes uneven, at others flocculent or fissured, and less dense than the deeper portion. In many cases the membraniform deposit is dry and of a granular appearance. Low forms of cryptogamic plants are occasionally seen upon it. Examined by the microscope, the membraniform deposit is found, according to Dr. Greenhow, to consist of coagulated fibrin and epithelium, the latter being most abundant in the outer layer, the deeper portion being more purely fibrinous. Exudation cells are often intermixed with the fibrillated texture. In some cases the deposit is more or less stained with blood. From being simply opaque, it soon becomes white or ash coloured, or if thick and adherent, brownish or buff coloured; when stained with blood, blackish. It is sometimes very loosely, at others very firmly adherent to the subjacent surface; occasionally, especially when friable and granular, it is merely superimposed upon the natural surface.

The mucous membrane upon which the pseudo-membranous deposit occurs is often intact; generally it is much congested and swollen. Sometimes it is white, opaque, or unnaturally pale, or, it has a raw appearance, the epithelium having been shed with the false membrane. It often has an excoriated and roughened appearance, is occasionally ulcerated, and more rarely gangrenous. When the adherent deposit is raised up, it is often seen to be attached to the surface beneath by numerous small filaments, as though processes of the deposit passed into the mucous follicles. When removed, in such cases, the mucous membrane is more or less dotted with bloody points.

"The submucous tissue is often œdematous, infiltrated with blood, and sometimes the seat of interstitial exudation. The tonsils are usually swollen, and on being cut into, are often infiltrated with blood, so as to impart to them an ecchymosed appearance; sometimes their tissue is softened. In two instances Dr. G. found the centre of a tonsil in a state bordering on gangrene. There is generally more or less inflammatory effusion into the structure of the tonsils. In one instance, on the tonsil being laid open, there was an oozing from it of a creamy fluid resembling pus. In some instances, the œsophagus and the muscular and other tissues around the fauces are congested or infiltrated with blood. The parotid and submaxillary regions are much swollen, and the integuments studded with livid purpureous spots."

In cases where the pseudo-membranous deposit extends to the larynx and trachea, it generally diminishes in thickness and firmness as it descends into the air-passages, until it assumes finally the form of a very thin pellicle or of a creamy fluid. There is usually more or less congestion of the mucous membrane of the affected portions of the larynx and trachea. It is often thickened, also, so as to diminish the calibre of the passage, even after the false membrane has been removed or has separated. The same remarks are true in reference to the subjacent mucous membrane here as were made when speaking of that of the fauces. The epiglottis, besides being covered on its upper and lower surface with false

membrane, is often swollen so as to contract the entrance to the windpipe. The bronchial tubes are sometimes lined with the diphtheritic deposit to the third or fourth bifurcation, or even beyond. The lungs are sometimes emphysematous, or they may be affected with pneumonia, usually of the lobular form. The kidneys are sometimes entirely healthy, in other cases they have been found congested, and presenting under the microscope when sliced, transparent fibrinous casts of the tubes, which occasionally contain blood-corpuscles, or granules of hæmatine, or a few altered epithelial cells.

There is a very general agreement among those who have had an opportunity for studying diphtheria, as to the necessity of a tonic and stimulant treatment from the very onset of the disease. It has been recommended, and we think with propriety, to commence the treatment when the patient is seen immediately when attacked by the administration of a prompt emetic of sulphate of zinc, followed by a moderate purgative of castor oil and turpentine.

The most efficient internal remedies are Watson's chlorine mixture, or, a simple solution of chlorate of potass in syrup and water, with a minim of dilated hydrochloric acid to each grain of the salt, given with the tincture of sesquichloride of iron; the dose being in each case proportioned to the age and condition of the patient, and repeated every three, four, or six hours. This mixture Dr. Greenhow prefers in cases unattended with urgent symptoms, and where the deposit is firm, and there is no tendency to hemorrhage. When the mucous membrane surrounding the deposit is deeply injected and softened, abrading and bleeding on the slightest touch, or whenever there is albuminuria, he considers the tincture of the sesquichloride of iron alone, in full and frequent doses, to be the proper remedy. If there is a tendency to purpura, he recommends the addition of from five to ten or twelve minims of diluted hydrochloric acid. When the hemorrhagic tendency coexists with great fetor of breath, the tincture of sesquichloride of iron should be given in conjunction with the chlorine mixture.

When there is great depression from the onset of the attack, ammonia given in decoction of cinchona is sometimes very useful, but in most cases it will be found inferior as a stimulant to good wine, or to brandy combined with egg or milk. The quantity and repetition of these must be regulated by the state of the circulation, care being taken to so time their employment and regulate their amount as to prevent, both day and night, any rapid or considerable flagging of the pulse. In cases occurring in young children a resort to suitable nutriment, will render the use of any large amount of alcoholic stimulants unnecessary, and by proper management the patient may almost always be got to swallow nutritious substances. The best of these are beef-tea, properly prepared; chicken broth, new milk, cream, and fresh laid eggs soft boiled, or, in cases where alcoholic stimulants cannot be dispensed with, beaten up with wine or brandy.

In the croupal form of diphtheria, as soon as the embarrassment of respiration is observed, and the patient's strength will admit of it, an emetic of sulphate of zinc and ipecacuanha should be given and followed by small repeated doses of calomel or hydrargyri c. cretâ; nutriment and stimulants being at the same time given freely and regularly, so as effectually to sustain the patient's strength. In these croupal cases we have found a combination of calomel, muriate of ammonia, ipecacuanha, and extract of hyoscyamus often to afford very prompt and decided relief. All external applications to the throat, poultices, fomentations, rubefacients and blisters, do far more mischief than good. As convalescence becomes established, fresh fish, oysters, chicken, tender-loin steaks, mutton chops or other solid food, nutritious and easy of digestion, should be given as early as possible.

The patient throughout the attack of diphtheria should be confined to his bed, and until convalescence is confirmed, he should still retain the recumbent position. Sudden movements, and active exertions of every kind should be avoided until the strength has become fully re-established. Even for some time after recovery has been apparently confirmed, it will be prudent to give a little light nourishment, or a glass of wine and water, each time before active exercise is taken.

In respect to local treatment, whatever may be the benefit derived from the direct application of nitrate of silver, or of a slightly diluted tincture of the sesquichloride of iron, or from the use of a gargle of the latter in a still more diluted form and sweetened with honey, in the early stage of the attack, when there is but a small amount of deposit and that on the anterior portion of fauces, at a later period of the disease, they are, to say the least of them, in most cases of very doubtful propriety; in many instances they are absolutely and to some extent injurious. In all cases mild, soothing detergent applications will, as a general rule, be productive of the most good.

"Simple gargles of borax or alum dissolved in water, or of solution of chloride of soda, in the proportion of half a drachm of the solution to the ounce of water, in either case sweetened with honey," are said by Dr. Greenhow, to "bring away the foul secretions and loose flakes of exudation, and thus, both by cleansing the mouth, and lessening the obstruction about the fauces, very materially to facilitate the administration of remedies and nutriment. Such gargles may be used either cold or tepid, as is most agreeable to the patient, and in very young children, or whenever patients are unable to gargle, they may be injected into the throat with a syringe, the patient's face being held over a basin immediately afterwards, so as to facilitate the return of the liquid together with the secretions and debris of the exudation which it may have detached. Probably in many cases, a simple injection of tepid water might answer the purpose equally well." Dr. G. has seen little benefit derived from the use of the chlorinated gargle, except where the breath was fetid from the decomposition of the exudation within the fauces. When the excretion has cleared away, and the throat is free from ulceration or excoriation, it is well to discontinue all local treatment, time being usually the best remedy. Sometimes the throat remains slightly congested or relaxed, or very sensitive to changes of weather for a long time after an attack of diphtheria. In the former of these cases the gargle, with the tincture of sesquichloride of iron already mentioned, has been the most useful application in Dr. G.'s experience. In the latter, he is accustomed to trust to constitutional treatment, gargling the throat with cold water, and, especially when the advice can be followed, to commend change of air.

The several forms of nervous disease which follow diphtheria usually disappear with time, but the cure is often promoted by judicious change of air and scene, good nourishment, and the use of tonics. Diluted nitromuriatic acid with the infusion of gentian or calomba, will often be found to suit the patient; citrate of iron and quinine also are often very useful. When there has been albuminuria or purpura, or the patient is extremely anemic, the tincture of sesquichloride of iron, with or without a few minims of diluted hydrochloric acid, or a grain or two of quinine to each dose, according to circumstances, is often serviceable. In some cases sulphate of quinia combined with diluted sulphuric acid, in any proper vehicle, will be found the best tonic. In cases where paraplegia has followed diphtheria, minute doses of strychnia are said to have been sometimes useful. "On the whole," remarks Dr. Greenhow, "though convalescence is usually much protracted, patients for the most part recover perfectly from the anæmia and purely nervous sequelæ of diphtheria, provided they survive the sixteenth or seventeenth day of their illness; and though tonics should certainly be persevered in, if they appear useful, there can be no doubt that they ought to be discontinued, if they do not agree with the patient nor appear to accelerate his recovery, seeing that time and good nursing will, in all probability, restore him to his pristine health." D. F. C.

ART. XXII.—*A Handbook of Hospital Practice; or, an Introduction to the practical study of Medicine at the Bedside.* By ROBERT D. LYONS, K. C. C., M. B. T. C. D., L. R. S. C. I., M. R. I. A., etc. Physician to Jervis Street Hospital, Dublin; Professor of the Practice of Medicine and Pathology in the Catholic University of Ireland; Late Pathologist-in-chief to the British Army in the Crimea, &c. &c. &c. New York, S. S. and W. Wood, 1861. 12mo. pp. 185.

A CONVENIENT manual for the guidance of the advanced student or hospital resident in those early clinical studies which are often the most important of a lifetime, must undoubtedly be a very useful book. We find, in the present volume, condensed into a small space, and expressed clearly as well as concisely, a great deal of that kind of information which the student has generally been expected to search for through immense volumes, or to receive in the perishable form of oral instruction. It may, therefore, serve somewhat the same purpose in "walking the hospital," or, in studying cases of disease in private practice, which is met in the dissecting room by the "Practical Anatomy" or "Dissector's Guide."

The first section of the work is occupied with directions for the clinical examination of patients. For classification, the system of nosological nomenclature of Dr. Farre, employed in the official registration of deaths in England, is adopted. This is by no means a perfect system; but will no doubt answer as well as any other as yet in public use. Diseases are, therein, divided into four classes, each with several orders; as follows: Class I. Zymotic Diseases. Order 1. Miasmatic Diseases. Order 2. Enthetic Diseases. Order 3. Dietetic Diseases. Order 4. Parasitic Diseases. Class II. Constitutional Diseases. Order 1. Diathetic Diseases. Order 2. Tubercular Diseases. Class III. Local Diseases. Order 1. Brain Diseases. Order 2. Heart Diseases; the remaining orders comprising Lung, Bowel, Kidney, Gennetic, Bone and Muscle, and Skin Diseases. Class IV. Developmental Diseases. Order 1. Of Children. Order 2. Of Women. Order 3. Of Old Age. Order 4. Of Nutrition.

A very good practical account is next given of the best method of scrutinizing the whole condition of a patient, as concluded upon from the knowledge of I. The history of the patient antecedently to the time of the examination. II. His state at that time; comprising all particulars concerning, 1. His general, moral, and physical condition. 2. State of cerebral functions and organs. 3. State of pulse and other general phenomena of the circulation. 4. State of respiration. 5. State of general cutaneous system. 6. State of primæ viæ, digestive system, and urinary system. 7. State of thoracic cavity and organs, including the special diagnosis of diseases of the heart and great vessels, lungs, and pleuræ. 8. State of abdominal cavity and organs. 9. State of the cutaneous system. 10. Malformations, and wounds or injuries of any kind, affecting the head, trunk, or extremities. III. Therapeutic appliances. IV. Daily progress of the case. V. Termination of the case.

It is well remarked by the author, in connection with this schedule, that the method of clinical examination and record laid down in the well known work issued by the London Medical Society of Observation, under the title "What to Observe at the Bedside," is too abstractedly scientific to work with at the bedside, although it is very useful to form the mind upon in the study. The present Handbook has been composed especially with the intent of avoiding this abstractedness and impracticability, and of making the student's knowledge immediately available. There is thus afforded a very brief though comprehensive summary of the principal facts in symptomatology, physical diagnosis, chemical and microscopical examination of the urine, &c. In short, it is an epitome of Medical Diagnosis, compressed into about 110 duodecimo pages. The subjects of pathology and treatment of disease are not touched upon. Morbid anatomy, however, receives full attention, in the second section of the volume; which, to the extent of 52 pages, is entirely occupied with directions

for the post-mortem examination of all parts and organs of the body. This section will be especially valuable, both to the student and practitioner; as it contains detailed as well as general information, given just in the order and manner in which they are wanted for use.

An Appendix is added, with brief directions for writing prescriptions, and a tabular view of the principal signs, abbreviations and Latin words employed in pharmaceutical language. A Glossary follows, chiefly of Latin and Greek words used in medicine; and the volume concludes with a few blank Forms for reporting cases, according to the system inculcated in the text.

The writer's experience has evidently made him acquainted with the wants of the clinical student, and has enabled him to prepare a book for which we anticipate a popularity in proportion to its merit. H. H.

ART. XXIII. *Compendium of Human Histology.* By C. MOREL, Prof. agrégé à la Faculté de Médecine de Strasbourg. Illustrated by 28 plates. Translated and edited by W. H. VAN BUREN, M. D. New York: Baillière Brothers, 1861. 8vo. pp. 207.

THIS work, which is intended to serve as an introduction to the study of human histology for the medical student, is especially to be commended, as attempting to embody the more recent observations of German and other investigators. Thus, for example, in the section on cells, we see with pleasure Schwann's erroneous doctrine of free cell-development thrown aside, and we are correctly told that "every cell must derive its origin from another previously existing cell. In the present state of science, but two modes are known in which cell-generation is accomplished in human histology: *endogenous* generation, and multiplication by *cleavage*." (p. 14.)

So also we are presented with the more modern doctrine of the structure of connective tissue (called connecting tissue in the translation), in the account of the development of which certain of the errors of the earlier microscopists are, however, retained.

"The essential elements of connecting tissue are fibres and cells. Its fibres are of two kinds, viz: connective fibres properly so called, and elastic fibres. Its cells are diminutive in size, generally branched, but sometimes fusiform, and have received from Virchow the name of plasmatic cells." (p. 17.)

And again: "The cellular element of connecting tissue (the plasmatic cell) is a recent discovery; we are indebted to Virchow for the first thorough exposition of its nature, and especially of its important pathological relations. Plasmatic cells are minute corpuscles, sometimes fusiform, but more frequently star-shaped, with sharp outlines, and connected with each other by means of their branching prolongations, so as to constitute a network similar to that formed by the cells of bone." (p. 19.)

"Finally, the researches of Virchow tend to prove that all, or nearly all, of the morbid formations developed in the meshes of the connecting tissue throughout the body, are traceable to the perverted growth of plasmatic cells." (p. 21.)

The above quotations sufficiently show the views of M. Morel with regard to the minute structure of connective tissue, as well as the source from whence they are derived. We are decidedly of the opinion that it would have been better had he drawn from the same authoritative source his account of its development. According to Virchow, while some of the embryonic cells are assuming the stellate character of connective tissue corpuscles (plasmatic cells), and others becoming elastic fibres, &c., the homogeneous intercellular substance in which the elements are imbedded becomes fibrillated in appearance, and thus produces the connective tissue bundles. According to M. Morel, however, the connective tissue bundles are developed out of cells after the fashion first described by Schwann.

"The fibres of connecting tissue develop themselves from cells of the simplest

form, which commence the process by assuming an elongated shape, then join each other, end to end, and gradually break up into fibres within, so that each row of cells thus attached by their extremities is developed into a bundle of connective fibres." (p. 23.)

Besides this method, M. Morel thinks he has observed in pathological cases the transformation of a similar series of cells into a single fibre, and also the transformation into a single fibre of an elongated series of free nuclei; as to the first of these possibilities, we feel convinced that the observation on which it is based was concerned with the development of yellow elastic fibres, with which the drawing of the specimen well accords (Plate IV, fig. 4); as to the second, we are not able to gain a satisfactory idea of the basis of fact upon which it reposes, or to interpret the drawing of the case (Plate IV, fig. 5).

The erroneous views entertained by M. Morel of the genesis of the connective tissue bundles, necessitates a similar error in connection with fibro-cartilage. "In the formation of fibro-cartilage a portion only of the original formative cells take on the changes above described, whilst the remainder transform themselves into connective and elastic fibres." (p. 29.) This is a view of the development of fibro-cartilage which has no foundation in fact, and which is at once overturned by the practical study of the development of this tissue; the matrix of the fibro-cartilages being homogeneous at first, and subsequently fibrillating, so that these fibres, like those of the white fibrous bundles, represent, in fact, not cells, but transformed intercellular substance.

It is not in our power, nor do we feel inclined at present to follow M. Morel from subject to subject in detail. Did we deem it advisable to do so, we should in many other points be obliged to criticize him for not presenting to the student the best information attainable at the present moment. Indeed, any error in the selection of his views on disputed points becomes especially important, where, as in the present work, the compendious character of the treatise compels the author to admit no other account than that which he determines upon as his own.

Perhaps, however, the most serious fault of the book is its extreme brevity. An account of the minute anatomy of the organ of smell, begun and finished in about half a page, of the mammary gland in six lines, of the Meibomian glands in three, &c. &c., is not complete enough to give anything but the loosest general notions of the matter to a novice, and can be of no possible use to the more advanced student.

In concluding, it is only just to bestow the praise which is deserved upon the American translator, who has moreover added a number of valuable notes, and to commend the general character of the lithographic plates, twenty-eight in number, with which the book is illustrated. J. J. W.

ART. XXIV.—*Recherches sur la Substitution Graisseusse du Rein.* Par M. le Docteur ERNEST GODARD, Ancien Interne des Hôpitaux de Paris, Membre de la Société de Biologie, etc. Paris, Victor Masson, 1859. 8vo. pp. 31.

M. GODARD occupies the first few pages of his monograph with citations from several authors, to the effect that fat may be deposited in the tissue of various organs, a fact which we believe is not disputed. He next impresses upon the reader the view that fatty infiltration or deposition, is quite a different thing from fatty degeneration, or transformation—a task which he might well have spared himself, as we are not aware that any opposition exists among pathologists to this very obvious and reasonable doctrine.

In the next place, M. Godard asserts that the kidney is, after the muscles, the organ most liable to undergo fatty degeneration, or substitution. We think this opinion admits of being questioned; nevertheless, no doubt can exist that fat is frequently substituted for the normal tissue of the kidney. Several cases are cited by M. Godard from the writings of Morgagni, Rayer, Cruveilhier, Petrequin, and others, and he concludes that

"The cases which exist in science, and which I have collected, appear to me to show that fatty substitution of the kidney is essentially characterized by the deposit of a variable quantity of fat, either in the parenchyma of the gland, or at its periphery, but always within the capsule or in the hilum of the organ. Then the fat penetrates between the mucous membrane and the pyramids, and leads little by little to the atrophy of the kidney."

After giving the details of an autopsy which came under his own notice, and quoting a case from Baader, and one reported by Rayer, M. Godard says:—

"The two preceding cases afford some information relative to the condition of the patients, in which, after death, fatty substitution of the kidneys was discovered.

"I shall not endeavour, whilst recalling the principal facts which they present, to trace the history of this disease; in order to do this, I am of the opinion that more complete, and more voluminous testimony is necessary. I will insist only on the difference existing between the infiltration of fatty granulations and globules in the epithelium of the kidney—an occurrence which is often met with in albuminous nephritis—and fatty substitution of the same organ, which results from, and coincides very frequently with, the presence of one or many calculi in the ureter, or pelvis of the kidney.

"I think that these two diseases are distinct, and that the one cannot be transformed into the other.

"Whilst the existence of the first is made known by the occurrence of easily recognized conditions (albumen in the urine, ascites, and anasarca), the other supervenes insidiously, and may lead to the more or less complete destruction of one of the kidneys, without exciting suspicion of its presence. The case observed by M. Bricheteau shows that if both kidneys are affected, the secretion gradually diminishes, then stops for a certain number of days, and the patient dies without presenting any of the symptoms which are the ordinary consequences of the sudden suppression of the urinary excretion."

This passage closes M. Godard's memoir—a memoir in which we do not find an original idea, and which belongs to a class unhappily becoming very numerous, especially in France.

The essay is illustrated with one steel plate engraving, representing fat globules, adipose vesicles, and several tissues affected with fatty substitution, which, like the text, is made up of borrowed material—Bowman, Johnson, and Robin, being put under contribution for the purpose. There are also two lithographs, showing the appearance of the kidneys, in the case of which M. Godard had the opportunity of witnessing the autopsy. W. A. H.

ART. XXV.—*Anatomy of the Arteries of the Human Body, descriptive and surgical, with the Descriptive Anatomy of the Heart.* By JOHN HATCH POWER, M. D., etc. With Illustrations, by B. Wills Richardson, F. R. C. S. I., etc. 12mo., pp. 374: Dublin, Fannin & Co., 1860.

ALTHOUGH specially designed for the use of students, and of practitioners who have not frequent opportunities for dissection, this excellent little volume may be consulted with advantage by any one who has occasion to investigate matters relating to its subject. The descriptions contained in it are extremely clear, concise, and accurate; equal perhaps to those of any of the more imposing treatises on anatomy. They have evidently been prepared by one who not only knew what to say, but how to say it, having both a natural facility and an acquired skill in teaching.

Dr. Power's production is a very much modified edition of a book bearing the same title, by the late Dr. Flood, of Dublin. The latter work, which enjoyed a good share of popularity in its day, is now out of print; it has been partly curtailed and partly amplified, and its arrangement slightly changed, in preparing it for reissue. We are somewhat inclined to regret the omission of the introductory sketch of the physiology and comparative anatomy of the circulatory

system, prefixed to the earlier editions, and to wish that Dr. Power, instead of striking it out, had reproduced it with such corrections and new facts as to make it accord with the science of the day. Nor do we think his transference of the anomalies to a concluding chapter an improvement upon the usual method of describing them in connection with the normal anatomy of each part; this is, however, a matter of individual taste. We are glad to notice that he has been careful to recognize the efforts which Godman, Gibson, Mott, Peace, and other American practitioners have made for the advancement of the anatomy and surgery of the arteries.

The typographical execution of this little work is handsome, and very generally correct. More than sixty wood-cuts, many of them original, have been introduced in preparing this edition; but these seem to our eye to have been drawn better than they were engraved. Those relating to the heart, taken from the work of M. Jamain, are by no means equal to the originals, and many of the others are at the same time small and coarse.

We should be glad to see a good reprint of this monograph placed in the hands of the profession in this country; it would doubtless meet with a cordial reception. J. H. P.

XXVI.—*On Gout; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M. D. Fourth Edition. 8vo. pp. 430. London, 1860.

THE very high estimate we gave of the treatise of Dr. Gairdner, upon the appearance of the first edition, has been, we are pleased to find, fully sustained by all who have subsequently given a public expression of their opinion of the work. The views of the author in respect to the pathology of gout, furnish, it must be admitted, a far more probable explanation of the phenomena and results of the disease, in different cases, from its onset onwards, throughout its entire course, than any of those previously advanced, and a more consistent rationale of the treatment, whether preventive or curative; whether directed to the eradication of the depraved diathesis upon which the attack of gout essentially depends; to the alleviation of certain urgent symptoms, or to the special management of individual cases, in the particular circumstances under which these usually present themselves to the notice of the practitioner. The treatise is throughout highly instructive; and, on many questions of deep interest, concerning the nature and causation of gout, and its therapeutics, is eminently suggestive. The fact that the demand for it has been such as to have exhausted already three editions, is a significant indication of the esteem in which the treatise is held by the physicians of Great Britain, while its translation, by professional men of eminence, into other European languages, evince its equal appreciation by the physicians of the Continent.

Having already, in our notice of the first and second editions of the treatise, given a tolerably full analysis of the views advanced by its author, and of the leading facts and arguments by which these are sustained, the same task need not be gone over on the present occasion.

To the edition before us the author has added such observations in illustration and enforcement of his teachings, as his subsequent more enlarged experience has suggested.

A chapter has been added, devoted to a consideration of the mode of application of the principal remedies adapted for the cure of gout to individual cases, having regard to the age of the patient, his constitution, and the period of the attack. The remarks which are made in respect to the treatment of the metastatic forms of gout are pertinent and instructive. We have seen quite enough of the disease to convince us that, in many of these cases, the practitioner will produce far more mischief by prompt interference, and a resort to heroic remedies, than were he to remain altogether inactive, or, at furthest, to merely attempt by the simplest and mildest measures, the alleviation of the immediate and more urgent sufferings of the patient. We can fully endorse the remark of

Dr. Gairdner that, in a large number of instances of metastatic gout, if time be given, and forbearance practised, it is wonderful to observe what painful and alarming symptoms will quietly depart with very little artificial aid. Time, as he justly remarks, is of great importance in the treatment of misplaced gout. However grave appear the circumstances, it is always unwise to prescribe in hurry and alarm, the seeming risk being often much greater than the actual danger, so that recovery is frequently retarded rather than prompted by the powerful remedies which are too readily and too commonly resorted to.

"At present it is a matter of very nice tact and judgment to distinguish the case in which the most heroic remedies must be instantly applied to sustain the fleeting powers of life, from that in which the advantage of the patient is best consulted by '*expectation*' and forbearance; but of one thing Dr. G. feels certain, that medical practitioners will best exhibit those qualities which entitle them to confidence, when they do not suffer themselves to be hurried into decision by the clamor of patients and their friends. In general, it may be said that those cases attended with little change in the pulse, even though there be intense suffering, severe vomiting, and signs of much disturbance in the stomach and diaphragm, best bear delay; and that those in which the action of the heart is very depressed, and where there is little acute pain, are the cases which most imperatively call for prompt and effectual aid."

The edition before us contains the notes of the German translator of Dr. G.'s treatise, Dr. Braun, of Wiesbaden, whose large experience, and intimate acquaintance with German physiology and pathology, give to them no trifling interest. D. F. C.

ART. XXVII.—*Further Researches on the Gray Substance of the Spinal Cord.* By J. LOCKHART CLARKE, Esq., F.R.S. (Reprint from the Philosophical Transactions, Part I., 1859.) London, 1859. 4to., pp. 31. With seven lithographic plates.

IN this paper Mr. Clarke has given us the elaborate results of his investigations into the minute structure of the posterior cornua of the medulla spinalis.

"Many of the older anatomists," he says, "from Bartholinus downwards, had observed that the gray substance of the spinal cord is softer, more delicate, and more vascular than the surrounding white columns; but Rolando was the first to point out a diversity in its structure. He observed, chiefly in quadrupeds, that on each side the posterior third of the gray crescent consists of a peculiar cineritious substance, which presents a different aspect from that which forms its two anterior thirds: it is different in colour, darker and less red. Rolando, however, assigned too large a space to this 'new substance,' which does not comprise so much as the posterior third of the gray crescent, but forms only a comparatively narrow and curved lamina or band around the extremity of each cornu, and, when viewed in a thin section by transmitted light, is found to be actually much paler and more transparent than the rest of the gray crescent."

Our author divides each posterior cornu into two parts, namely, the caput cornu and the cervix cornu. The caput cornu consists of two different portions, an outer and comparatively transparent portion, called the gelatinous substance, and an inner and more opaque portion or base. The gelatinous substance consists of nerve-fibres, nerve-cells, bloodvessels, and connective tissue with nuclei.

The nerve-fibres are transverse, longitudinal, and oblique. The primitive fibres composing the bundles of nerves are tubules of small, average size, the larger kind possessing double contours. In size the greater number vary from about the $\frac{1}{2500}$th to the $\frac{1}{2000}$th of an inch in breadth, but are intermixed with others of about the $\frac{1}{1700}$th of an inch in diameter.

The cells of the gelatinous substance are described by Mr. Clarke as large, small, and intermediate. The large cells are almost exclusively found among the semicircular fibres which run within the external border of the substantia gelatinosa, and never extend beyond the middle of its depth. They are more or less oval, pyriform, fusiform, crescentic, triangular, or otherwise irregular, and

contain distinct nuclei. They all give off processes from two to five or six in number. The small cells abound in every part of the gelatinous substance. They are round or oval, somewhat pyriform, fusiform, or angular, and give off two processes or more. They vary considerably in size, and many are nearly as minute as blood-disks. The largest of them contain distinct nuclei. The cells of intermediate size are found chiefly near the verge of the posterior columns.

The inner or more opaque portion of the caput cornu also consists, besides bloodvessels and connective tissue, of longitudinal, transverse, and oblique fibres and cells.

Upon the microscopic anatomy of the cervix cornu Mr. Clarke dwells at considerable length. He devotes several pages also to the description of the gray substance of the filum terminale and the epithelium of the central canal.

For the benefit of those who may be engaged in similar investigations, we transcribe Mr. Clarke's method of preparing parts for microscopic examination.

In an appendix to the paper under consideration he says: "The structure or part intended for examination should be as fresh as possible, and cut into portions as small as is compatible with the end in view. These portions I formerly hardened by means of a mixture of one part of spirit of wine and three parts of water, which at the end of twenty-four hours was replaced by a fresh mixture of equal parts of spirit and water, and this again after the same interval was replaced by pure spirit, which ought to be renewed every five or six days. At the end of ten to fourteen days the medulla is sufficiently hard for making sections, which are then subjected to the following process for the purpose of inducing transparency. The sections are first placed in a mixture of one part of strong acetic acid, and four, five, or six of spirit, for a period varying from two or three to ten minutes, according to their thickness. They are then washed in pure spirit, after which, if thin, they are floated on the surface of spirit of turpentine, where they remain until they are quite, or nearly transparent, when they are removed to glass slides on which a little Canada balsam has been previously dropped. If now examined under the microscope, they frequently show but little or no traces of cells or fibres; but if the sections be set aside for some time, and treated occasionally with a little turpentine and Canada balsam, the cells and fibres reappear and present a beautiful appearance. Before they are finally covered with the glass, they should be examined at intervals by the microscope. If the sections be thick, I find it best to place them in a shallow vessel, the bottom of which is kept simply wet with turpentine, which can therefore ascend from below while the spirit evaporates from their upper surfaces; for the *principle* of the method is this: to replace the spirit by turpentine, and this by Canada balsam, without drying the sections. The method at first presents some difficulties, and practice is necessary to insure complete success. Experience, also, will suggest, according to circumstances, many little deviations from the exact rules here given, which, to a certain extent, must be considered as general. For the last three years I have used chromic acid instead of spirit in the process of hardening. The medulla of man and the higher mammalia is steeped in a solution of one part of crystallized chromic acid in two hundred parts of water, for two or three weeks, and then kept in a solution of about one part of bichromate of potash in one or two hundred parts of water. Spirit is used to wet the knife in making the sections, which are first placed in spirit for a few minutes, and then (with or without the previous use of acetic acid) transferred to the turpentine and Canada balsam, as before." J. A. M.

ART. XXVIII.—*Etudes sur la Rage dans divers Etats de l'Europe, et particulièrement dans la haute Italie.* Par M. BOUDIN. Annales d'Hygiène publique, &c., Janv. 1861.

IT is a popular notion that hydrophobia, or rabies canina, occurs almost exclusively during the hottest season of the year. This idea is both suggested and perpetuated by the laws which require dogs to be muzzled during that period

alone. On the other hand, the greater number of medical writers upon this subject have reiterated the statement that rabies is quite as apt to occur under one extreme of temperature as another. Among ancient authors who describe the disease, none make an allusion to its peculiar prevalence during the summer. Cælius Aurelianus, who has left so graphic and correct a picture of rabies, has not a word of its relation to temperature. Pliny, indeed, says *rabies canum sirio ardente homini pestifera;* which implies that rabies is peculiarly dangerous in summer, but is not so at that season exclusively. Paulus Ægineta, after stating that dogs for the most part become mad during violent heat, adds: but, also, as Lycus says, sometimes in extreme cold. Aëtius writes that this affection is most frequent where wide extremes of heat and cold prevail.

Among modern authors who have insisted upon the occurrence of rabies in very cold as well as in very hot weather, may be mentioned Boerhaave and his commentator Van Swieten, Mease, Villermé, and Troillet. All are agreed that mere temperature is not an adequate cause of the disease, because in high northern and in tropical climates it is of rare occurrence; and some, like Bardsley, infer from the latter fact that atmospheric conditions have no share in its production at all. It is evident, however, that one or both extremes of temperature may serve as an exciting cause of this or any other affection which depends upon a specific virus. And, again, its prevalence at a particular time may depend, as Hertwig remarks, upon the mere accident of a mad dog running at large, and inoculating a large number of persons and animals. Thus, in May, 1817, a single mad wolf, in the neighbourhood of Lyons, bit twenty-three persons, thirteen of whom died of hydrophobia.

In his valuable "Report on Hydrophobia," Dr. Blatchford makes the following remarks: "Every investigation, and anywhere made, only proves that a belief in the influence of the 'dog-star,' or climate, or season of the year, as inducing or favouring the production of rabies, is an utter fallacy, a mere astrological fancy, handed down, it may be, from remote ages, its very antiquity, perhaps, shielding it from the probe of investigation; but yet entirely unsupported by facts." Now, it will have been remarked in the citations above made, that the doctrine here advocated has always been held by medical writers upon rabies; they have uniformly maintained that no season enjoyed an immunity from the ravages of the disease, and have generally agreed that either extreme of temperature is favourable to its production. How, then, are we to account for the popular belief, or superstition, whichever it may be?

While in doubt upon this point, we met with the paper of M. Boudin, of which the title is prefixed to this article, and finding that his results, the largest which ever have been collected, fully sustain the current notion of the greater prevalence of rabies during warm weather, it seemed not uninteresting to extend the research into other periods and localities than those examined by him. Having divided the year into two halves—as nearly as the reports would allow—one of which included the warm and the other the cold months, two tables were constructed, showing the number of persons bitten, and also the number who became hydrophobic during the respective periods. The following are the results:—

Persons Bitten.

Authority.	March to Aug.	Sept. to Feb.
Blatchford[1]	52	49
Smith[2]	57	50
Radcliffe[3]	45	39
Boudin[4]	526	280
Tardieu[5]	76	41
	756	459

Or, 1.64 : 1

[1] Trans. Am. Med. Assoc., ix. 257.
[2] New York Journ. of Med., N. S., xv. 241.
[3] Lancet, Feb. 1855, p. 153.
[4] Annales d'Hygiène, Jan. 1861, p. 183.
[5] Ibid., Jan. 1860, p. 199.

Authority.	*Date of Attack.* March to Aug.	Sept. to Feb.
Blatchford	47	41
Smith	48	59
Radcliffe	40	30
Tardieu (1854)[1]	67	30
Tardieu (1860)[2]	110	71
	312	231

Or, 1.35 : 1

It appears, then, that the number of persons bitten by mad dogs during the warm months is two-thirds greater than during the cold months of the year; and that the proportion attacked with rabies in the former period is one-third greater than during the latter. These facts are sufficient to explain the popular belief upon the subject, while they, at the same time, demonstrate how many rabid animals and cases of hydrophobia in man are to be met with at seasons when there is but little apprehension of the disease. It may, perhaps, be suspected that the excess of wounds from rabid animals during the warm months is due to the greater number of vagrant dogs at this season, rather than to the greater relative proportion of cases of rabies among these animals; but, on the other hand, it is chiefly at this period that precautions are taken, through police regulations, to diminish the canine population, and, by means of muzzles, to prevent dogs from biting. A. S.

ART. XXIX.—*Cours théorique et pratique de Braidisme ou Hypnotisme, Nerveux considéré dans ses rapports avec la Psychologie, la Physiologie et la Pathologie et dans ses applications à la Médecine, à la Chirurgie, à la Physiologie Expérimentale, à la Médecine Légale et à l'Education.* Par le Docteur J. P. PHILIPS, suivi de la relation des Expériences faites par le Professeur devant ses élèves, et de Nombreuses Observations par le Docteurs Azam, Braid, Broca, Carpenter, Cloquet, Demarquay, Esdaile, Gigot-Suard, Giraud-Teulon, Guérineau, Ronzier-Joly, Rostan, etc. Lapis quem reprobaverant ædificantes. Paris: J. B. Baillière et Fils, 1860. 8vo. pp. 180.

IT is a well-known fact that if we place in front of, and at a little distance from, the eyes of any impressible person, some brilliant object, as a piece of glass or polished metal, and cause the attention of such person to be concentrated upon the object, or if we make passes before his eyes with the hands in such a manner as strongly to rivet his gaze, he will presently fall into a cataleptic state, analogous to that produced by the inhalation of certain anæsthetic agents. While in this condition, his limbs are either rigid, or more or less completely relaxed: general sensibility is blunted, or even abolished, and certain of the special senses, such as hearing and touch, become remarkably acute. The faintest sounds are perceived, the lightest impressions are vividly felt; and dreams and hallucinations, as in certain cases of catalepsy, offer themselves in quick succession to the mind.

These singular phenomena—though within the past year they have attracted and are still attracting much attention in Paris, owing to their practical applications in surgery—are by no means new. Under the name of *actinobolism*, they were long ago described by Kircher, in his *Ars Magna*. A writer named Schwenter, also dwells upon them in a work published in 1656, and entitled *Deliciæ Physico-Mathematicæ*. They appear to have formed an important element in the religious ceremonies of antiquity. According to Bernier and Anquetil Duperron, the mystical Joguis of India were accustomed to hypnotize themselves by looking fixedly, for hours at a time, at the end of the nose, or at some imaginary point in space. We are also informed that the Christian monks

[1] Ibid., Jan. 1854, p. 230. [2] Ibid., Jan. 1860, p. 205.

of Mount Athos, the Arabians of the sect of Aïssa, and the Egyptian magicians, threw themselves, at will, into this cataleptic or trance-like state, by somewhat similar procedures. The phenomena of hypnotism, or Braidism, constituted the basis of the medical system of the Brahmins, the Therapeutæ, the Asclepiades, and the mystical physicians of the school of Alexandria. They explain, in a measure, some, at least, of the prodigies performed by ancient and modern thaumaturgists, and they afford an experimental confirmation of those singular recitals which occupy so considerable a place in the annals of the middle ages concerning the works of enchanters, magicians, sorcerers, &c.

In the 16th century, the examination of these phenomena was undertaken in a critical spirit. Cardan, Maxwell, Wirdig, Paracelsus, Van Helmont, and others, endeavoured to bring them into the category of general physics. More recently, Mesmer gave to the world the startling results of his investigations. In 1843 appeared the remarkable work of Braid, entitled *Neurypnology, or the Rationale of Nervous Sleep, considered in relation with Animal Magnetism.* This was followed, in 1845, by his *Observations upon Trance*, and in 1852, by another publication upon *Witchcraft, Hypnotism, Electro-Biology*, &c. Since the time of Mesmer, various observers, of greater or less note in the medical and scientific world, have directed their attention to this subject. Among these we find Cloquet, Rostan, Huston, Lordat, Reichenbach, Frank, Jobard, Gregory, Esdaile, Lafontaine, J. K. Mitchell, J. H. Bennett, Simpson, Carpenter, Alison, Holland, Sir D. Brewster, Dugald Stewart, Charpignon, and others. Quite recently, Dr. Azam, of Bordeaux, having long and patiently experimented upon hypnotism, announced to the physicians of Paris that he was able to produce catalepsy and anæsthesia at will, during which surgical operations, ordinarily very painful, could be performed without suffering to the patient. Attracted by these experiments, Drs. Bazin, Broca, Demarquay, Velpeau, and others, have also been lately engaged in examining this subject. The results of their investigations are recorded in the *Gazette Hebdomadaire*, the *Archives de Médecine et de Chirurgie*, *Gazette des Hôpitaux*, *Bulletin Général de Thérapeutique*, *Journal de Genève*, *Gazette du Midi*, and other journals.

The work before us is one of the latest contributions to the science of hypnotism. It is enthusiastically written, and contains the details of many experiments and observations of a very singular and anomalous character. Its opening pages are occupied with a brief account of the origin and progress of Braidism. This is followed by a description of the process by which this condition is produced, of the phenomena by which its different stages or degrees are characterized, and of the dangers which sometimes accompany its induction. The physiology of hypnotism is next considered, and finally many observations and experiments are brought forward, illustrative of its medical, surgical, and medico-legal applications. These observations and experiments constitute by far the most satisfactory and reliable portion of Dr. Philips' work; and they are calculated to attract the attention of those of our readers who are at all interested in this curious field of inquiry. J. A. M.

ART. XXX.—*Lives of Eminent American Physicians and Surgeons of the Nineteenth Century.* Edited by SAMUEL D. GROSS, M. D., Professor of Surgery in the Jefferson Medical College of Philadelphia. 8vo. pp. 836. Philadelphia: Lindsay & Blakiston, 1861.

THE series of biographies presented in this volume comprises those of some of the most distinguished of the physicians and surgeons who by their knowledge and skill, their success as teachers, cultivators and practitioners of the healing art, have illustrated their profession, and conferred honor upon their country.

Among the elder of our medical and surgical practitioners were many men of great learning—close observers and acute interpreters of the various morbid conditions of the animal organism—fruitful in resources for their control, and

skilful in the adaptation of these to the several phases of morbid action presented in individual cases. Of the professional career of these gentlemen, the story, taken in connection with the circumstances amid which their lives were passed—their respective characters, personal habits, and domestic and social surroundings, constitute a theme which, when fitly dealt with, is as instructive as it is interesting.

The lives of some of those we have just had reference to, have been ably and fully delineated; of others we have as yet but imperfect records, and of many none at all, while in the mean time the materials necessary for their preparation are fast disappearing.

Early in the present century an effort was made by Dr. James Thatcher, of Massachusetts, to collect the biographies of the more distinguished of our older physicians, and as far as the effort extended it was attended with marked success. Some years later the same thing was attempted, by Dr. Stephen W. Williams, likewise of Massachusetts. Neither of these works, nor the two together, cover the entire ground of American medical biography. The number of distinguished medical men of our country, whose lives are omitted, far exceed that embraced in them. Nor does the volume before us supply, to any great extent, the omissions of Thatcher and Williams. There are still materials sufficient for many more volumes of American Medical Biography, even though the life of no one should be admitted whose demise occurred later than the end of the first quarter of the current century.

The volume under notice is an exceedingly interesting one. The lives contained in it, with scarcely a single exception, are written with ability, and commendable impartiality, presenting a very vivid and spirited delineation of the character and services, the professional and social status of their respective subjects. But three of these memoirs—those of Drs. John Syng Dorsey, Ephraim McDowell, and Daniel Drake are from the pen of Dr. Gross, the others have been derived from other sources—either from the personal friends of those who are their subjects, or qualified, at least in a special manner, for the delicate and responsible office of their biographers from an intimate acquaintance with their career, their labours, and their character. The plan is an excellent one, and better calculated to insure a just delineation of character than were the entire series of biographies prepared by a single individual.

Several of the lives in the present volume—those of Hosack, McClellan, and Hartshorne—have been prepared by the sons of their respective subjects. The task, which is confessedly one of unusual delicacy, has been performed in the instances before us, with commendable taste and judgment.

The memoirs presented in the work of Dr. Gross are arranged in chronological order according to the period of decease of those whom they respectively commemorate. A somewhat awkward plan, which gives Drs. Dorsey, Godman and Eberle precedence of Dr. James Thatcher, a practitioner only a very few years the junior of Dr. Rush.

The work of Dr. Gross is in all respects deserving of a favourable reception, not only from American physicians generally, but from the community at large. It is one well adapted to popularize the character and office of the medical practitioner, by placing his educational requirements, his professional services, and his claims upon his fellow citizens in a proper light before the public.

D. F. C.

ART. XXXI.—*A Catalogue of the Pathological Cabinet of the New York Hospital.* Classified and arranged by ROBERT RAY, Jr., M. D., Curator. With a Memoir of the author. 8vo., pp. 364. New York, S. S. and W. Wood, 1860.

EVERY large hospital where clinical instruction is given should have attached to it a pathological museum, in order that the demonstrations of disease in the living subject may be illustrated still further, and the ante-mortem and post-

mortem phenomena placed, as it were, side by side. With most men, and perhaps especially in dealing with such a science as medicine, the memory retains impressions made upon the eye better than those which fall upon the ear. Hence, where students are to be practically taught in the elements of their profession, and in many cases to receive their preparation for an immediate entrance upon its active duties, it is important that the lecturer should be provided with specimens exhibiting the causes, as well as the meaning, of symptoms. A striking instance of this is found in diseases of the chest; a teacher can explain abnormal heart or lung sounds to infinitely greater advantage by means of specimens, or even diagrams, showing the lesions met with, than by mere description.

The New York Hospital was, we believe, the first institution of the kind in this country to act upon this idea; its museum was first opened in 1841. The example thus set has been recently followed at the Pennsylvania Hospital, as well as at the Baltimore Infirmary. Our impression is that at nearly all the English hospitals, as well as at some of those in Germany and France, more or less extensive pathological collections have been made.

As a matter of course, a full descriptive catalogue greatly enhances the value of every such collection. Very few specimens in morbid anatomy are self-explaining, and often our conclusions are essentially modified by the knowledge of certain circumstances in the antecedent history of a case. A catalogue may, indeed, be so prepared as to be of great use even to those who have not access to the collection itself, and especially to such as are labouring in a similar field. The largest and best work of this kind is probably the catalogue of the splendid museum of the Royal College of Surgeons, in London, arranged by Messrs. Stanley and Paget. Equally well known, or perhaps more so, from the frequency with which they are quoted, are the two octavo volumes and the atlas of exquisite lithographic plates published by the Parisian Faculty of Medicine, describing and illustrating the bony preparations contained in the Musée Dupuytren.

The catalogue whose title heads the present notice, although a far more unpretending work than those just mentioned, bears evidence of great care and judgment in its preparation, and contains much that will prove interesting as well as useful to pathologists. Its author, whose talents and industry were bidding fair to win for him a high professional rank, was called from all earthly labour before this volume had been given to the press; and his former preceptor, Dr. Watson, has prefixed to it a short sketch of his life.

It would of course be impossible to enter into anything like an analysis of a work, one of the chief objects of which is conciseness of description; suffice it to say that the catalogue of the Museum of Guy's Hospital was the model adopted in framing it, and that the collection has been classified into eight sections. These are (1) the bones; (2) the joints and tendons; (3) the digestive system; (4) the respiratory system; (5) the circulatory system; (6) the nervous system and organs of special sense; (7) the genital and urinary systems; (8) parasites. Under these heads are arranged descriptions of nearly one thousand specimens, with references attached to many of them, by which their histories may if desired be still further traced. By the regulations of the New York Hospital, the house physicians are obliged to keep ample records of all the cases treated in the institution; and in this way a very large mass of experience has been accumulated. The pages of these case-books are often referred to in the catalogue of the pathological cabinet.

We cannot but regard the publication of this volume as an important step in the advancement of medical science in America, and as a fresh recognition of the value of morbid anatomy. Dr. Ray's last work will have gained for him an enduring remembrance among his professional brethren, should it prove the earnest of greater things in the cultivation of pathology in our hospitals; and such we trust it will. J. H. P.

ART. XXXII.—*Report of the Board of Health of Philadelphia, for* 1860; *Sanitary and Statistical. In accordance with an Act of Legislature, approved March 8th,* 1860, *for the Registration of Births, Marriages, and Deaths.* Philadelphia, 1861. 8vo. pp. 81.

THIS is an interesting document. Though somewhat superficial and defective, it nevertheless recommends itself to the notice of every citizen, inasmuch as it deals with matters which concern his safety from disease, suffering, and death. The duties intrusted to the Board of Health place it among the most important of all our municipal bodies. Presuming that those who compose it have a just appreciation of the object of their appointment, and an intimate acquaintance with the best means for its accomplishment, and that the Board is clothed by law with ample power to carry into full effect whatever sanitary reforms it shall, after due deliberation, and with all the facts necessary to the formation of a correct judgment before it, deem necessary and proper—its operations cannot fail to become an efficient agent in securing the health, promoting the comfort, and enlarging the prosperity of the community. Upon it the indwellers of the city may then safely rely for the prevention of every avoidable disease in their midst, by guarding against its causes, removing promptly such as are present, and preventing, if possible, their recurrence in future.

In the report before us, we are presented with the first fruits of the law for the registration of the births, marriages, and deaths occurring within the limits of the city of Philadelphia, which went into operation, under the auspices of the Board of Health, on the first day of July, 1860. The returns under this law, as given in the report, embrace, consequently, a period of six months. These, although insufficient of themselves to serve any valuable purpose in reference to the vital statistics of our metropolis, have been properly tabulated as reliable data for the preparation of future reports, to which we must look for materials adequate to the construction of a correct sanitary history of Philadelphia.

After disposing of the subject of registration, and the manner in which it has been inaugurated under the provisions of the law of 1860, the report hastily glances at some of the leading questions connected with the all-important theme of civic or public hygiene, with the view of directing the attention of the citizens of Philadelphia, and their municipal representatives and agents, to the necessity of their correct and prompt solution, and the carrying into full effect the principles and procedures thence deduced. In the leading views and suggestions embraced in the report, we fully concur; from the correctness of certain of the details given by the committee from whom the report emanated, we must, however, entirely dissent. Thus, we deny that the present system of *surface drainage*, even when regulated and aided by the best and most extended system of grading and paving, can ever become, in the words of the report, "well adapted to carry away the fluid refuse from our streets, houses, and yards, and to answer as conduits (?) for the rain which falls upon our city." In this climate, throughout every season of the year—summer and winter alike—it is an intolerable nuisance, and should be replaced, as quickly as opportunity presents and the financial means of the city are in a condition to warrant it, by a system of underground drainage, through conduits and culverts, of sufficient size and extent, and constructed upon strictly scientific principles.

We must express our surprise at the action of the Board of Health, as set forth in the report, in relation to the City Hospital; an institution destined expressly for the reception of patients affected with contagious or infectious diseases, who cannot be properly attended at their own residences, or in any other of our public or private hospitals, without endangering the spread of the disease to those in the same institution with them, or even beyond its walls; an institution which had been pronounced necessary by the medical and municipal authorities of our city over half a century ago, and has been maintained ever since, by each successive Board of Health, under all the various modifications in organization it has undergone. Great as is our surprise in reference to the action of the Board in closing the hospital referred to, it is even more so when we consider the reasons

presented in the report before us in justification of that action. The City Hospital has been shut up, we are informed—1st, because there is no law which makes it obligatory on the Board to maintain a hospital for the reception of patients other than those for whose benefit ample accommodations are secured at the Lazaretto; 2d, because of the large increase of private-hospital accommodations throughout the city; 3dly, because of the revolution of medical opinion in respect to the communicability and mode of propagation of many of those diseases for which the City Hospital was specifically intended; and, lastly, because the outlay for the support of the hospital has been altogether disproportionate to the benefits that have been derived from it.

In considering the validity of any or all of the foregoing reasons. we must keep steadily in mind the leading object for which the Board of Health has been organized, as well as certain of the means imperatively demanded for carrying out that object by circumstances that not only may possibly occur at any moment, but which have occurred again and again, and are at this present time in actual existence.

If a hospital for the reception of patients labouring under an infectious or contagious disease is one of the means necessary to prevent the spread of such disease in any particular locality or neighbourhood, or throughout the community, the power to open and maintain a hospital for that purpose exists in the Board of Health. Even were it not conferred by the very letter of the law under which the Board acts, it is most unquestionably by its spirit. Not only, we insist, does that law *allow* the Board to maintain a hospital of the character referred to, but it *imperatively requires* it to do so.

We will suppose that a case of smallpox—over which disease the Board of Health exercises the same jurisdiction as it does in respect to any other—occurs in an inmate of one of our hotels, boarding-houses, or prisons; in a domestic; or the disease shows itself among the occupants of a confined, overcrowded, and unwholesome neighbourhood; or among the crew or passengers of a ship that has just arrived at one of our wharves. In either case, if there is no hospital in charge of the Board of Health ready for the immediate reception of the infected person or persons, and within a reasonable distance from the centre of the city, to render it easy of access, they must be sent either to the Lazaretto, to the almshouse, or to one of the private hospitals of our city, or be left where they were found as so many sources of infection, endangering the spread of disease far and near. Now, this latter alternative the Board would be criminal in resting contented with—will they be able to adopt either of the others?

The project of sending smallpox patients out of the city to be treated at the Lazaretto, is one that should not be entertained. It would cause some delay in preparing the means necessary for their transit, when prompt removal is always of importance. The distance, also, of the Lazaretto from the city, the difficulties which lie in the way of their comfortable and safe transportation there, and the expense that would be incurred by this manner of disposing of those, affected with smallpox and other contagious or infectious diseases, are reasons sufficiently weighty to place it out of the question.

The idea of sending smallpox patients to the almshouse, is equally preposterous. In the first place, the Board of Health have no power to allow the Guardians of the Poor to dispose of cases of malignant or contagious disease, even when these occur in paupers, excepting in a manner perfectly satisfactory to it, and under the immediate supervisorship of its officers; for it is an express duty of the Board of Health to guard against the introduction and spread of pestilential and contagious diseases. It cannot itself send, nor permit others to send, patients affected with these diseases to the almshouse, because of the danger which would thereby be incurred of spreading the disease throughout its crowded wards. Were a properly constructed building—ample in size and perfectly isolated—to be provided by the Guardians of the Poor, there would, of course, be no valid objection to its being made use of as "a pest-house" for paupers—but for paupers only. The idea of sending to such an institution any one who is able and willing—or his friends for him—to pay all expenses incurred for his support and treatment in an institution separate and apart from that appropriated to the accommodation of paupers, is preposterous in the extreme, and should receive no countenance from any quarter.

It is very true that, in consequence of the change in medical opinion in respect to the origin and propagation of many diseases heretofore classed as infectious or contagious, the same isolation of these as was formerly believed to be necessary is no longer insisted upon. Hence, those affected with yellow, typhoid, and perhaps typhus fever, in some of its forms at least, may not be excluded from the wards of any of our general hospitals; but we know of no one among the many admirably arranged and skilfully conducted institutions of this kind which exist in our midst, that would be willing to admit into its wards a case of the mildest form of smallpox. Even were any one of them to become so far derelict to the duties it owes to the patients placed under its care as to be willing to introduce among them a case of smallpox, it would become the imperative duty of the Board of Health to prevent this being done. We are to recollect that it has been constituted the curator of the health of the poor as well as of the rich —of the pauper as well as of the millionaire. It is the province of the Board to watch over the sanitary condition of the prison, the almshouse, and the hospital; of the confined neighbourhoods and hovels where dwell the humblest and most abject of our population, as well as of the open streets and ample mansions inhabited by the proud and the wealthy.

We believe that a hospital, properly planned and judiciously administered, exclusively appropriated for contagious and infectious diseases, under the jurisdiction and control of the Board of Health, is an institution which cannot, with safety, be dispensed with. Occasions are constantly occurring in which the want of such a hospital would necessarily entail much discomfort, if not actual suffering, upon not alone the pauper classes of our community, but upon respectable citizens, whose only misfortune is that they have become the subjects of an infectious or contagious malady, or that their families are threatened with the invasion of such disease, in consequence of their vicinity to those labouring under it and amid circumstances well adapted to favour its spread.

The large outlay required for the support of the City Hospital is one of the reasons urged for its discontinuance. We confess that we are unable to perceive how the expense to the community is to be in any great degree lessened, if at all, by shutting up the City Hospital, provided the Board of Health is true to its obligations to protect the community from the spread of contagious or infectious diseases. Every time the latter makes its appearance in our midst, there will be a necessity for the removal and isolation of cases. These must be taken charge of by one or other of the municipal bodies, and become, necessarily, a public charge. We are in favour of every judicious reform having for its object the curtailment of the present enormous expenditures of our city government, but we are, at the same time, fully convinced that every movement calculated to jeopard the health and lives of the community, no matter what may be the *apparent* pecuniary saving effected by its inauguration, would become, in the long run, wasteful in the extreme. D. F. C.

ART. XXXIII. *Theory and Practice of the Movement-Cure: or, the Treatment of Lateral Curvature of the Spine; Paralysis; Indigestion; Constipation; Consumption; Angular Curvatures, and other Deformities; Diseases incident to Women; Derangements of the Nervous System; and other Chronic Affections—by the Swedish System of Localized Movements.* By CHARLES FAYETTE TAYLOR, M. D. With illustrations. Philadelphia: Lindsay & Blakiston, 1861. 12mo. pp. 295.

ALTHOUGH it would be neither right nor expedient to affix to the code of medical ethics a sweeping prohibition of all specialization of professional labour, yet it is only a sound conservatism that throws a heavy *onus probandi* upon every new, or newly systematized, method which proposes to displace long recognized modes of practice, and that denounces, unconditionally, all schemes of *exclusive* therapeutics.

Dr. Taylor's work on the "Movement-Cure" is, in the preface, addressed to

the medical profession; and, notwithstanding the unavoidable prepossession against it produced by the title itself, we have thought it right to give it a fair and attentive reading. It is a plausible and not uninteresting exposition of the system of medical gymnastics, originated by Ling, of Sweden, who died at Stockholm in 1839. The name of *Kinesipathy* was at one time given to it. The author's reading has been well adapted to the handling of his subject, and his claims on behalf of the system are advanced without much pretension. Yet, with all candour, the best we can say of it is, that its truths are trite, in the knowledge of the profession at least, if not out of it; and that its novelties in principle have the disadvantage of being unsound, while those of detail are comparatively unimportant.

The essential idea of the book is, that passive exercise, localized in particular groups of muscles, can be made to act with voluntary movements, and often to more decided advantage than they, in controlling and modifying the local conditions of nutrition, so as to promote the cure of chronic diseases. The two radical elements in the pathology of organs containing muscular tissue are considered to be, *retraction* and *relaxation*. These are to be opposed and remedied, respectively, by favouring the entrance of arterial blood into the capillaries of the region affected, or by assisting the exosmotic escape of fluid from the tissue into the capillaries and veins. The former is accomplished by *eccentric*, and the latter by *concentric* muscular manipulations and movements.

The hypothesis at the base of this reasoning involves at least two very doubtful statements; that any decided influence upon the flow of arterial blood into a part is exerted by stretching the muscles passively, *i. e.*, the operation being performed by another than the patient himself; and, secondly, that any important action upon the venous circulation can be maintained by the reverse process of passive flexion of the limbs or trunk. These propositions comprising the whole essence of the movement-cure, so far as it is at all novel or peculiar, we are forced to consider it unsustained by any well-grounded physiological reasoning.

Many of the remarks in Dr. Taylor's book upon the general physiology of exercise, and upon popular errors in hygiene, are sound enough, and sufficiently well expressed. If the first part of the work, without the above-mentioned hypothesis, had been issued as an essay upon such subjects merely, we might have thought it likely to be useful. But, when we study the application of the scheme to the treatment of disease, such an opinion cannot be entertained. In lateral curvature of the spine, it is true, the principle of invigorating the non-symmetrical muscles has been long familiar to surgeons; the merits or demerits of particular appliances used, is a topic of secondary consequence. In paralysis, the quotation by Dr. Taylor from Dr. Todd, shows that exercise has been well understood to have a certain value. The mechanical pullings and twistings of the kinesipaths must, it appears to us, have a vastly less exhilarating effect in this disease, and still more in dyspepsia, consumption, and nervous disorders, than riding on horseback, or walking, with those who are able, or driving or sailing with weaker invalids. Nor do we believe that they possess one whit of advantage over ordinary gymnastics, pursued merely upon the idea of developing all the muscles, and promoting the circulation in all parts of the system. It is possible to carry out the details of the plan of treatment described in this work only in an establishment fitted up for the purpose, with apparatus and assistants dedicated to it. Will the superiority of this medication, if it can be called such, be commensurate with the preparation required? We should prefer the Turkish bath, or the *thermæ*, recently introduced into England, as auxiliary to appropriate medical treatment. The danger of such specialties is, moreover, that, in proportion to the absence of real and tangible advantage in the scheme, is the inducement for overweening enthusiasm to take its place, and to fortify itself by denunciation of regular medicine, and by efforts to impair the confidence of the public in all measures but its own. For these reasons, we are obliged to confess that, notwithstanding the interest of many of the subjects treated of in Dr. Taylor's book, we cannot regard it, in a practical aspect, as likely to confer benefit either upon the profession or upon the community at large.

H. H.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On Pulmonary Osmosis, or Researches on the Absorption and Exhalation of the Respiratory Organs.* By Louis Mandl.—In the author's researches on the intimate structure of tubercles (published in the *Archives* for 1855), he arrived at the conclusion that these morbid productions are devoid of all organization, consisting solely of a solid, amorphous substance, the result of a coagulation of matter first dissolved in the blood and then exuded. He compared this amorphous element with analogous products of various diseases, and demonstrated that pathological histology does not furnish sufficiently distinctive characteristics.

Continuing these researches, and examining into the causes which give rise to tubercular exudation, the author has arrived at the conclusion that it is sometimes due to a general cause—a diathesis—and at others to a purely local or accidental cause. In the course of the investigation, he was struck with the frequency of the coincidence of tuberculization and diabetes; and he sought to ascertain the influence of glucose, spread throughout the organism and penetrating every tissue, upon the production of tubercle. This has led to the performance of a series of experiments, an account of which it is the object of this paper to give. The experiments consisted in placing certain aquatic animals in saccharine solutions. The following is a *resumé* of the results:—

1. The life of animals breathing in water is incompatible with the presence of a more or less considerable quantity of saccharine matter, whether this consist of the various kinds of sugar or non-fermentable sweet principles, such as glycerine or mannite. 2. Infusoria, pulmonated mollusks, annelides, crustacea, batrachians, aquatic insects, and fish, all perish in a period more or less short, according to the kind of sugar, the strength of the solution, and the species of the animal. Thus, fish, from twelve to fifteen centimetres in length, die in forty minutes in a solution containing a tenth part of glycerine, and live four or five hours in a solution of sugar of the same strength. 3. Numerous experiments have proved that death cannot be attributed to poisoning, to chemical action on the blood, to fermentation, to absence of air, or to viscosity—it being solely due to the *osmosis* (endosmosis and exosmosis) induced by the saccharine solution. 4. This action is excited through the permeable membranes, and especially through those of the respiratory organs; the non-fermentable sweet principles possessing superior osmotic power to that of the true sugars, which explains the celerity of their action. Infusoria comport themselves as vesicles, the osmosis operating through the whole of their delicate envelop; and they are seen first to collapse (exosmosis) and then to distend (endosmosis), even sometimes to bursting. In animals of higher development, in which the thickness of the integument confines the osmosis chiefly to the branchiæ, the blood is observed

to become thickened in these, and the circulation arrested in them, in consequence of the exosmosis of the liquid portions. The circulation in the lungs of a frog may be instantly arrested, over a limited space, by the application of a drop of glycerine, and in a few minutes by a drop of syrup. Experiments made on the fœtus of the salmon, and on the circulation of plants, have furnished similar results. 5. Many hundred experiments were tried with the endosmometer, having various substances (the pericardium of the sheep being the most suitable) for its diaphragm, and it resulted that the water of the blood, charged with the salts of the serum, first passed into the saccharine solution, next the albumen, and then the colouring matter. The passage of all these elements took place in a short time, when only a small portion of the osmogenous substance lay on the diaphragm; but the passage of the albuminoid elements becomes slow in proportion as the quantity of the saccharine fluid is considerable, so that, for a long time, the water containing the salts is then all that passes through. 6. Development is also impeded by saccharine solutions, as is shown by experiments made with muscular tissue macerated in them, and by those instituted on the fertilized ova of fish. 7. The following physiological and pathological circumstances are elucidated by the foregoing investigation: (*a.*) Infusoria, the excessively thin envelop of which is very sensible of osmotic action, can neither live nor become developed in fruits impregnated with sugar, as plums or grapes; but insects with their thicker integument may become developed in apples, plums, etc. Doubtless, it is by the thickness of its envelop that the cysticercus is protected from the osmotic action of the glucose in the liver. (*b.*) The antiseptic and preservative power of sugar, known for so long a period, may be explained by its power of arresting the development of organized beings. (*c.*) The thirst induced by the ingestion of sugar is due to the exosmosis of the serosity of the blood. (*d.*) The abundance of glucose in diabetes explains the characteristic thirst in this disease, and the impossibility of any serous effusion. (*e.*) The coincidence of gangrene with diabetes has been frequently observed, and may not the arrest of circulation, leading to gangrene, be attributed to the existence of glucose in the vicinity of the capillaries? (*f.*) The absence of venereal desires and of procreative power observed in the diabetic may be due to the presence of glucose in the semen—this substance in the experiments causing the immediate death of spermatozoa. (*g.*) Small quantities of sugar induce an exosmosis of the gastric juice, and thus powerfully aid digestion in some persons; while large quantities introduced into the blood increase the osmotic power of the fluid; and this explains the utility said to have been derived from the exhibition of sugar in dropsies. (*h.*) The advantageous employment of glycerine as a dressing in gangrene, hospital gangrene, etc., is explained by the impossibility of the development of organisms in pure glycerine. Purulent and other serosities are resorbed by the glycerine; and the utility of the application of this substance to dry surfaces arises from the osmosis establishing a greater humidity. (*i.*) Various experiments have been undertaken in reference to the local production of tubercle by the injection of osmogenous substances, and especially saccharine substances, into the pulmonary vesicles. The results, which, when more multiplied, will be published, give good reason for the belief in the production of localized tubercles in the lung by the exudation (exosmosis) of plastic matters, independently of diathesis. These experiments bring the subject back to its original starting-point—the accidental production of tubercles in diabetes, in which affection all the tissues are impregnated with glucose. These inquiries open a new field to therapeutics, in explaining the (in the author's belief) frequent spontaneous or artificial cure of tubercles of the lung, when these are independent of diathesis.—*Med. Times and Gaz.*, Jan. 12, 1861, from *Archives Générales*, tom. xvi.

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2. *Experiments on some of the various Circumstances influencing Cutaneous Absorption.*—WALLER's researches on cutaneous absorption lead to results which appear to us important with regard to practical medicine. The subjects of the experiments were guinea-pigs and albino rats. When the leg of a half-grown guinea-pig was immersed into a mixture of equal parts of chloroform and tincture of aconita, the part was after fifteen minutes insensible, and the symp-

toms of poisoning by aconite soon followed—viz., nausea, efforts at vomiting, coldness of surface, weak circulation, laborious respiration, slight convulsive symptoms, and death. The immersion in simple tincture of aconite without chloroform did not cause any of the symptoms of poisoning. If, however, the sciatic nerve had been previously divided, then the immersion of the leg in the simple tincture of aconite was sufficient to poison the animal, evidently through the distension of the capillaries and the more rapid flow of blood caused by the division of the nerve. A ligature placed round the limb before the first symptoms of poisoning had appeared, prevented the toxic influence on the system, but it rarely did so after the appearance of the earliest symptoms. In albino rats the immersion of the leg in a solution of atropia in chloroform caused dilatation of the pupils already after two minutes and a half in young animals, after five minutes or later in old animals. If turpentine was substituted for chloroform, the dilatation of the pupils did not occur while the leg remained immersed in the fluid, but appeared immediately after the removal of the limb. The substitution of alcohol for chloroform as a solvent causes great retardation of absorption. An immersion of twenty to thirty minutes produced only very slight effects. Still more slow is the absorption when the atropia is dissolved in water with a slight addition of acetic acid, or when the watery extract of belladonna is rubbed over the leg. The immersion of the foot of a young rat in a solution of morphia in chloroform caused after about five minutes somnolency and great dilatation of pupils, which latter phenomenon certainly must appear very remarkable when we consider the effect of morphia, given in the usual way, on the pupil. Three minutes' immersion in a solution of strychnia in chloroform produced dilatation of the pupil, and after five minutes, the well-known symptoms of strychnia-poisoning manifested themselves. No effect was obtained when a solution of strychnia in alcohol was employed. These experiments clearly show how important is the choice of the menstruum in the endermic application of some medicinal substances.—*B. and F. Med.-Chir. Rev.*, January, 1861, from *Proceedings of Royal Society*, vol. x.

3. *Excretion of Kreatinin and Kreatin through the Urine.*—Schottin, having during several years bestowed much attention on the circumstances under which kreatin and kreatinin appear in the urine, considers that these substances are chiefly derived from the tissue-change of the striped muscles. The author inclines to the view, that kreatin and kreatinin pass by further oxidation into urea. In healthy urine he found, while merely vegetable food was consumed, no kreatin or kreatinin, and only a very small quantity under the influence of an almost entirely animal diet. In many diseases, too, these substances were not found; in some pathological conditions, however, in which either the transformation of these bodies into urea appeared to be prevented (uræmia), or in which the waste of muscular tissue was increased (as in typhoid fever), the amount of kreatin excreted by the kidneys ranges between 0.2 and 1.1 grammes (about 3 to 17 grains) within the twenty-four hours.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Archiv f. Heilkunde*, 1860.

MATERIA MEDICA AND PHARMACY.

4. *On Indian Hemp, particularly in relation to its property of producing Sleep.*—Dr. Fronmuller first employed the Indian hemp in the case of a phthisical patient in the year 1850, and since that time he has devoted himself to the especial study of the properties of this substance. The result has been the production of a treatise founded upon the clinical observation of no less than a thousand cases in which Indian hemp was administered. This plant has been very much extolled by many practitioners in various countries, but has lately fallen into disuse, owing to the supposed uncertainty of its operation. The discredit attached to it is attributed by Dr. Fronmüller partly to the contradictory

statements published concerning its operation by various writers, and partly to the difference in its effects in the Eastern hemisphere compared with those observed in Europe. The Indian hemp of India and that grown in Europe present the same external form, but they differ in the relative proportion of narcotic resin which each contains, and which is the active principle of the plant. It appears that the amount of resin depends not only upon differences of latitude, but also upon the depression or elevation of the regions where the plant is grown. Chemical analysis has discovered that the Indian hemp contains gum, bitter extractive matter, albumen, chlorophyll, ethereal oil, and a peculiar resin. This resin is called *cannabin* by some writers, and forms six to seven per cent. of the dried plant. The ethereal oil has been obtained by Martius only in small quantity; it is of a slightly yellowish colour, of a peculiar ethereal camphor-like smell, and an aromatic astringent, and afterwards bitter taste. The preparations of Indian hemp hitherto employed are the powdered plant for pills or powders, resinous extract of hemp in powders or pills, tincture of the resin, and emulsion.

With regard to its application to the practice of medicine, Indian hemp may be considered valuable as a tranquillizing antispasmodic drug. It has been employed with favourable results in tetanus and trismus, cardialgia, rheumatism, and in some mental diseases. Of 1000 cases in which this drug was administered by Dr. Fronmüller, 552 were males and 448 females, and the patients were of various ages, from one year to fifty and more. The principal diseases of the patients were tuberculosis, inflammation, surgical diseases, rheumatism, diseases of the eyes, nervous diseases, and dropsy. The greater part of the cases were treated by the spirituous extract prepared by Merk in Darmstadt; but others were treated by the extract prepared by the late Jacob Bell, of London. It is to be observed that all the observations were made on cases in which there had been no sleep for several nights, and in which the continuance of sleeplessness was to be anticipated unless some narcotic was employed.

Out of the thousand cases it was found that the narcotic property of the hemp was completely developed in 530, partially in 215, and little or not at all in 255. With the extract of Indian hemp the best effects were produced 145 times with a dose of 12 grains, 64 times with a dose of 8 grains, 63 times with a dose of 10 grains, 35 times with 16 grains, 22 times with 3 grains, 17 times with 2 grains, 15 times with 14 grains, 14 times with 20 grains, 13 times with 6 grains, 12 times with 5 grains. The period of falling to sleep, and the duration of sleep in the cases, are numerically recorded by Dr. Fronmüller, and also the number of cases in which unfavourable results ensued on the day of taking the drug, or on the next morning. Comparative observations were also made with morphia in cases where the Indian hemp had failed. Out of 29 cases in which Indian hemp had produced no effect, sleep was produced by morphia in 24. The dose of morphia was in general rather a strong one—from one-sixth of a grain to 2 grains—in order to induce sleep. In the remaining 5 cases the morphia produced no effect.

The conclusions to which Dr. Fronmüller arrives as the results of his observations are the following: 1. That Indian hemp, among all the known medicines which cause stupefaction, is that which produces a narcotism most completely supplying the want of natural sleep, without occasioning any great excitement of the vascular system, without special stoppage of the secretions, without the supervention of unfavourable consequences, and without subsequent paralysis. 2. That Indian hemp, on the other hand, is not so strong nor so certain in its operation as opium. 3. That Indian hemp may given in all acute inflammatory diseases and in typhus fever. 4. That it is worth a trial to alternate the Indian hemp with opium in cases where the latter fails. 5. That the best mode of administration is the alcoholic extract in small pills which contain an addition of the powder of the Indian hemp. The lowest dose for producing sleep may be estimated as eight grains given in pills of one grain each.—*B. and F. Med.-Chir. Rev.*, 1861, from *Vierteljahrschrift für die practische Heilkunde*, 1860.

[It is proper to add a caution in regard to such large doses. If the extract be of the *best quality*, eight grains might not always be a safe dose.—ED.]

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5. *Oil of Turpentine as an Anæsthetic.*—Mr. JOHN WILMSHURST, in a communication in the *Lancet* (March 2, 1861), states that he has successfully

employed the ol. terebinthinæ rect. as an anæsthetic. The first case, he says, "in which I tried its effects was that of Mrs. H——, matron on board the emigrant ship *Indiana*, of which I was then surgeon-superintendent. About twelve months ago, having exhausted my little stock of chloroform, and the patient suffering from violent neuralgia in the course of the supra-orbital nerve, it occurred to me that of the remedies at hand the most likely would be the vapour of turpentine. This I immediately applied, sprinkled on a handkerchief, to the nostrils, similarly to chloroform, and was surprised to find it not merely soothe and allay the pain, but, after a few inhalations, produce a gentle sleep and state of anæsthesia, from which she awoke without any headache or other unpleasant symptoms, and quite free from pain.

"I may mention, without going into detail, that I have since tried it in one or two slight but painful operations—as extracting a broken needle from a sensitive part, and in some cases of cramps, convulsions, nephralgia calculosa, &c. Its effect seems to be to allay nervous irritation, spasm, and pain, without deranging the action of the heart, and to produce a calm anæsthetic sleep."

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6. *On the Therapeutical Employment of the Double Iodide of Iron and Quinine.*—Dr. Violette has given the double iodide of iron and quinine in several affections in which the preparations of iron have been recommended, and he has been enabled to prolong its use without meeting with the symptoms so often produced by the separate use of iron and quinine. He has never observed either weight at the stomach or offensive eructations, or gastralgia. In many affections of the uterus, after the local treatment has succeeded, the iodide of iron and quinine has seemed to restore the strength and revive the appetite. He has witnessed the same results in convalescence from typhoid fever, and from all diseases which induce a considerable impoverishment of the blood. Dr. Violette considers this medicine peculiarly efficacious in chlorosis. M. Becquerel allowed him to administer it to some young chlorotic girls, whose leading symptoms were complete loss of colour of the mucous membranes, a waxy complexion, excessive weakness, total loss of appetite, blowing murmur in the heart and carotids; in such cases Dr. Violette found that a rapid improvement followed the use of the double iodide. The appetite returned very quickly, and at the same time the strength was restored; a slight rose colour replaced the pale tint of the mucous membrane, and the extreme paleness of the face was changed for the natural colour.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Gazette des Hôpitaux*, July, 1860.

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7. *Kukui or Kekune Oil.*—Now that the oil of *Aleurites triloba* is spoken of so highly in France as a purgative oil, a few particulars concerning it may not prove uninteresting.

The plant producing the fruits from whence this oil is extracted, belongs to the natural order Euphorbiaceæ, and is plentiful in the Sandwich, Society, and other groups of islands in the Southern Seas. It is also to be met with in some parts of Jamaica and the East Indies. The oil has been for some time known in Jamaica as Spanish walnut oil, and in India as Belgaum walnut oil. In Ceylon the oil is called kekune oil, and in the Sandwich Islands kukui oil. The tree is known in some parts of Polynesia as the candle nut tree. The fruits are nearly as large as a walnut, and the kernel is inclosed in a thick, hard shell. These nuts are often strung together by the natives, and burnt, without any other preparation, as torches. In the history of the Mutiny of the Bounty it is stated, that the rooms in Pitcairn's Island were lighted up by torches made of "doodoe" nuts, strung upon the fibres of the palm leaf, forming a good substitute for candles. These nuts are also so strung and used by the San Blas Indians in Central America, and a child is in attendance to knock off each nut as it becomes burnt out.

The following is the method adopted in obtaining the oil in Jamaica. Each nut is carefully cracked or broken, and the kernel as carefully separated from the hard shell, lest the latter, having a brown dye quality, should affect the colour of the oil. The kernel is then put into a large mortar and pounded as fine as possible. It is afterwards thrown into a caldron with plenty of water

and boiled. It is allowed to simmer for hours, until all the oil is well extracted and floats on the surface. Meanwhile, and until all is gathered together, the oil is skimmed off into another clean vessel. The oil thus collected is then boiled over again in a smaller vessel for a short time, in order to throw off any aqueous particles remaining after the first skimming. If the oil is not then perfectly pellucid it is run through blotting paper. Eight quarts of kernels will yield about three pints of oil. The yearly produce of this oil in the Sandwich Islands is about 10,000 gallons. It has been shipped to the markets of Chili, New South Wales, and London, but, hitherto, without much profit. It realized about £20 per imperial ton in London. In 1843 about 8,620 gallons were shipped from Honolulu, valued at 1s. 8d. per gallon.

This oil has been used as an artist's oil, for which purpose it is said to possess valuable qualities, although it cannot be applied as a drying oil. It is only lately that attention has been called to its medicinal properties. It is purely purging, and, not like the croton, jatropha, caper-spurge, sandbox, and other euphorbiaceous oils, productive of vomiting at the same time. It is affirmed to be as mild as castor oil, and, being more fluid, is better to take. It is without either taste or smell.

The nuts have, within the past twelve months, been sold in the London market under the name of kekui nuts, and there is no doubt that, upon inquiry, some of the oil could be procured, and it evidently well merits the attention of the profession. A purgative oil which shall possess all the advantages, and none of the disadvantages of castor oil, is a desideratum worthy of being secured.—*London Med. Review*, Sept., 1860.

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8. *Glycerine.*— Dr. W. R. GORE, Surgeon to the Limerick Infirmary, states (*Dublin Med. Press*, Dec. 26, 1860) that he has been using glycerine for several years in the city infirmary with great advantage, and can confirm the observations of Dr. Jacob as to its utility in keeping soft the vegetable extracts when combined with it for external application. "Three or four years since it struck me that the tendency of belladonna to dry upon the skin could be obviated by admixture with glycerine, since which time I have been using it with all the advantage involved in such an object. I have been in the habit of applying a paste over the brow in those ophthalmic cases in which intolerance to light forms a prominent feature, composed of ext. belladonna, ext. valerian, pulv. opii, and acid hydrocyan, made into a soft paste with glycerine, which remains soft and absorbable to the last, and is very often most useful. I also use it with extract. conii and acid hydrocyan, as an internal application to the eye with belladonna or aconite, and with the most marked benefit in similar cases. In those strumous and other chronic affections of the cornea which render that organ defective in translucency, I have been using with advantage iodide of lead and atropine dissolved in this fluid, which is, in my opinion, one of the best media for the application of any of those active agents known to be useful in medicine. It keeps the part in a state more suitable for absorption, and permits the active agent to operate through it more readily than ordinary lard, oil, or ointment. We have found it very valuable in keeping dressings soft, pliable, and easy of removal. Sutures are kept, when painted over with it daily, in a similar state. Lotions are prevented from drying off too readily, and cloths moistened with it prevent too rapid an evaporation. In frictions it is the nicest possible thing to grease the skin, and in those obstinate scaly eruptions of the scalp it is most useful in softening the scab and rendering the medicinal agent operative. I do believe it will be found a most valuable agent in medical hands, applied as experience and ingenuity may dictate."

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9. *Honey and Glycerine in Surgery.*—Dr. C. F. MOORE, Medical Superintendent of Middleton Hospital and Infirmary, says (*Dublin Med. Press*, Dec. 26, 1860) that several months ago he commenced the use of glycerine to bedsores in bad fever cases, and finding it to exceed my expectations, he used it in all cases where a healing dressing was required. It occurred to him, some three or four months since, that honey might answer as well; and as a very great difference in price exists, that an important saving might thereby be effected by using an

equally efficient agent, "as some of the properties of honey previously known to me led me to think it to be. I have now used it in several cases in hospital and private practice, as in ulcers left by scalds, in a wound after the removal of a large tumour presenting some malignant characters from fascia of the leg in a woman of seventy-six years of age, in a case in which dead bone was being discharged after injuries to a man's leg, bedsore from fractured thigh, &c. &c. As I hope to bring the value of honey as a dressing more fully under the notice of the readers of your valuable journal, I will only now state that it seems especially useful in cases where a tendency is manifest in a wound or ulcer to throw up unhealthy granulations instead of healing kindly. It seems also to protect the skin adjacent by its own conservative agency, so to speak, as well as by correcting the sceptic properties of the discharges. In evidence of this latter property, I may state that, in cases which I have treated with it, after having used the more ordinary dressings previously, the honey most unquestionably removed all unpleasant smell from the discharges, itself no small gain. I must observe, however, that honey, when first applied especially, causes some considerable degree of pain, indicating its stimulating effects. In one case I used a wash of tinct. opii and water on first applying the honey dressing, poured over the fine linen which had been applied, saturated with fine clear pure honey. In all the cases, however, the patients themselves soon got reconciled to the pain, considering it indicative of benefit being received from the dressing. It is well, however, to bear this point in mind, especially in cases where a very large surface has to be dealt with, or a patient is very susceptible of or sensitive to pain. In such cases, glycerine, which possesses more soothing qualities, might be preferable. In conclusion, I would insist on the necessity of using pure clear honey quite free from any of the numerous articles said to be used in its adulteration."

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10. *New Method of employing Coal Tar for Medicinal and Hygienic Purposes.*—M. Demeaux, whose researches into the value of various disinfectants, conducted in conjunction with M. Corne, have acquired considerable reputation, has recently announced the discovery of a means by which coal-tar can be made up into the form of a soap.

This product, which, from the facility with which it may be prepared, from its moderate cost (which is less than sixpence for each pound), from the quantity of coal-tar which it contains, and from its ready solubility in water, appears destined to be of great service, is prepared in the following manner: Take of coal-tar, soap, and alcohol, equal parts, mix, and warm over a water-bath until entirely dissolved. Upon cooling, a perfect soap is obtained, which is very soluble in water and forms, upon being dissolved in that liquid, a permanent emulsion.

This preparation would be found very useful, whether in hospitals, in dissecting-rooms, or in manufactories, for the purpose of preventing real dangers to health, or of counteracting emanations which are both unpleasant and unwholesome.

The emulsion of coal-tar might be employed in baths or in lotions and fomentations to the body, and be productive of good results in certain diseases of the skin; body or bed-linen might be impregnated with it, and also dressings for patients whose excretions are of a fetid character. Its ready solubility in warm or cold water would prevent it from staining the body or the clothes.—*London Med. Review*, Feb., 1861.

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11. *Solution of Gutta-percha in Chloroform.*—Dr. W. R. Gore (Surgeon to the City of Limerick Infirmary) extols this as an extremely useful preparation. It is made by putting some cut-up shreds of gutta-percha into a small bottle of chloroform, which soon dissolve, forming a solution thick in proportion to the quantity of the gutta-percha. A good syrupy or mucilaginous consistence is best. Whenever it is desirable to exclude the atmosphere from a wound, to preserve the skin, or otherwise form a protective coating, there is no similar agent of equal value. It can be laid on with a brush, layer after layer, to form any required degree of thickness; it forms an admirable bond of union, and is entirely protective. The skin must be quite dry, and if possible, warm and dry.

Warm cloth kept for a few moments on the part will aid the process. There must be no oozing. Where there is much surface of wound, it is advisable to protect it with a little isinglass plaster or a bit of lint from the smarting effects of the chloroform, which, however, pass off when the first coat dries. If the coats are put on in too rapid succession it does not consolidate nearly so rapidly, nor is it so effectual as a bond. It adheres to the dry skin for a long time. The approach of a warm iron at any time softens it to a removable consistence, or it peels off in due course naturally.—*Dublin Med. Press*, Jan. 23, 1861.

12. *Purifying and Scenting Cod-Liver Oil and Castor Oil.*—M. JEANNEL, calling to mind the remarkable effect which bitter almonds exert in removing the odour of so powerful a scent as musk, has made various experiments on the removal of the disagreeable odour and taste of cod-liver oil. The results are: 1. That the essential oil of bitter almonds in the proportion of seven and a half grains to twenty-five drachms of even the most infected oil, is completely successful; the dose of the essence requisite varying with the degree of the fetidity of the oil. 2. One grain of anhydrous hydrocyanic acid dissolved in water, will disinfect, but does not perfume, the same amount of oil. 3. Distilled laurel-water is, however, the best means of effecting the result, it sufficing to shake well in a bottle, the oil with once or twice its volume of distilled laurel-water, according to the strength of this, and the degree of infection of the oil. The liquids are then to be separated by a funnel after forty-eight hours' rest; and if the oil is not sufficiently clarified, it may be rendered limpid by filtering through paper. This confers upon even the brownest oil a slight and pleasant flavour of bitter almonds, which abides in the mouth. Of course it does not remove rancidity, which is a very different thing from the fishy odour and taste. Three drops of essence of bitter almonds communicate a very agreeable flavour to twenty-five drachms of even nauseous castor oil, and render its administration easy.—*Med. Times and Gaz.*, Dec. 29, 1860, from *Journ. de Pharm.*, 1860.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

13. *Restoration of Suspended Animation in Persons apparently drowned.*—A paper on this subject, by Dr. CHRISTIAN, was recently (Jan. 22, 1861) read before the Royal Medical and Chirurgical Society.

Two societies, the Royal Humane Society and the National Life-boat Institution, issue instructions, which are widely circulated, as to the best mode of restoring suspended animation in persons apparently drowned. These rules differ not only in detail, but in principle. 1st. As to the mode of performing artificial respiration. 2d. As to the propriety of using the warm bath. On each of these matters the author stated his desire to elicit an expression of opinion from the Fellows of the Society, after laying before them some considerable experience acquired during twelve years as Medical Officer to the Royal Humane Society in Hyde-park. The number of cases of submersion for twelve years was 443. Of these, 181 were rescued and recovered without treatment; 165 were brought to and recovered by treatment at the Receiving-house; and 97 were brought dead or the treatment was unsuccessful. The number of cases of submersion for the last four years was 140. Of these 68 were rescued and recovered without treatment; 38 were brought to Receiving-house and recovered; and 34 were brought dead or were not restored. 15 of these cases were treated by the Marshall Hall Method, and 3 recoveries followed: the rest were treated by the rules mentioned below. As to the mode of performing artificial respiration, the method recommended by the Life-boat Institution is what Dr. Marshall Hall called his "Ready Method," while that now used by the Royal Humane Society is the method of Dr. Silvester. On Dr. Marshall Hall bringing his method under the notice of the Royal Humane Society, the committee

adopted means immediately to give it a fair trial. Copies of his instructions were sent to all their medical officers, numbering 120, and the boats of the Society on the Serpentine had a platform made to each, on which to manipulate directly the body was taken from the water, and the boatmen were instructed and practised in the performance of the operations as he directs. After giving the method a full trial in about fifteen cases, the very intelligent superintendent, the boatmen, and the author became so satisfied of its inefficiency to restore animation, and of the difficulty of properly carrying out the manipulations, that he felt himself justified in representing those facts to the committee, and in adopting the plan recommended by Dr. Silvester, which he believed in every way to be superior, more manageable, less likely to injure the patient, will fill the chest with and expel air from it more fully, and will not force the contents of the stomach upwards, and in the way of respiration. The following are the directions for treating the asphyxiated at the Receiving-house, Hyde Park:—"Wipe the mouth and nostrils directly the body is taken from the water. Use Dr. Silvester's method; at the same time let the body be taken as quickly as possible, to the Receiving-house, and place it in the bath up to the neck. Raise the body in 20 seconds from the water, and dash cold water against the chest. Pass ammonia under the nose. Use again Dr. Silvester's method, and the inflating apparatus if it fail. Remove the body from the bath, and rub the surface with dry hot towels, perseveringly continuing the other treatment." After many experiments the author had come to the conclusion, that inflation of the lungs by Dr. Silvester's method, or by the Society's apparatus, is the first remedy, and the shock of the warm bath the second; that, after eight minutes' complete submersion, recovery is hopeless, and that, when ten minutes elapse after being taken from the water without any effort at respiration, it is equally so. On the subject of the warm bath, which has excited so much discussion as a remedy, he remarked, that it must be understood that it is used as an immediate and powerful excitant; and it had so frequently happened (twice while he was actually present) that a person brought in as asphyxiated, who to the bystanders was apparently quite dead, immediately on being placed in the bath gave the sob or gasp which is the precursor of respiration, that it might be boldly stated to be a most valuable adjunct to treatment, and, properly managed, in no way pernicious. He concluded by citing a letter from Sir Benjamin Brodie to the Royal Humane Society, confirming the preceding conclusions.

Dr. Sharpey said that he had on one occasion spoken favourably of Dr. Marshall Hall's method, but after more mature consideration and after hearing the practical experience of the officers of the Royal Humane Society, he had found reason to alter his opinion. Dr. Sharpey considered that Dr. Marshall Hall's method could only claim one advantage; and it was not clear that it had even that. The supposed advantage being that the tongue falls forward and thus does not embarrass respiration. He thought that Dr. Silvester's method attained this object without any of the disadvantages of the "ready method." The disadvantages of the "ready method" were several. It must be remembered that a body which has been some time submerged is an insensible and inert body, and serious mischief has not unfrequently arisen from rough handling. Again, the constant turning of the body, renders it very difficult to apply warmth, or carry out the other auxiliary means systematically, but the greatest objection is, that it does not even fulfil its professed object, viz., that of changeing the air in the lungs. Dr. Marshall Hall had cited experiments in support of his views; but the want of precision in making them was very apparent from his description. He (Dr. Sharpey) could not attach any importance to the results of experiments so loosely conducted. Dr. Silvester had repeated them with greater care, and did not succeed in getting a displacement of more than a cubic inch of air. It would be said, however, What answer should be made to the many statements of the success of the ready method? He would reply that the subjects of many of these cases were still-born infants, and probably would have recovered without any assistance if left to themselves. Then, again, there is no air to be displaced in the lungs of infants. In reference to adults, he (Dr. Sharpey) believed himself right in saying that in many of the cases of recovery after submersion, respiration commences spontaneously soon after the

patient reaches the air. If in such cases Dr. Marshall Hall's method were begun at once, it would be unfair to give the credit solely to it. May it not even be, as suggested by Sir Benjamin Brodie, that recovery often follows, not from the means, but in spite of them? Dr. Sharpey considered that there were just grounds for reviving the old method of insufflation, which was, he thought, given up without sufficient reason. The objection to this plan was that it produced emphysema; but then, as he knew by experiment, this was from forcing in air too quickly and in too great quantity. Dr. Sharpey then alluded to the various instruments which might be used in insufflation. In reply to the objection that often insufflation could not be practised from the want of instruments, etc., he would remark that there could be no reason why the insufflation should not be carried on with the mouth, through a pipe put into one nostril of the patient. A card, or letter-envelop, might be made into a pipe till better was got. The force and pressure used would be no more than that sustained by the operator's own lung. He believed that emphysema in reference to these cases was a bugbear. Dr. Christian had observed that in unsuccessful cases in which insufflation had been practised, no emphysema was found at the autopsy; and a friend had told him (Dr. Sharpey) that he had never found emphysema in the lungs of still-born children who had been unsuccessfully treated by insufflation at the Edinburgh Royal Maternity Charity. The objection that the air would be deficient in oxygen was not practically valid, as the operator might, by several deep inspirations, quite change the air in his lungs, and then, by employing his chest for insufflation quickly after an inspiration, he would not leave time for any considerable change in it. There was an outcry against the warm-bath, founded on mere speculation. The Royal Humane Society's officers had found it to be very efficacious. Edwards and Dr. Brown-Séquard made their experiments on puppies and kittens, and their experiments were not, he believed, to be fairly compared with the use of the warm-bath in cases of suspended animation.

Dr. SIBSON had recently been engaged in prosecuting experiments on the restoration of suspended animation by the "ready method." They were only eight in number, and were not yet complete. In only one of them did the "ready method" appear to answer. The experiments were not complete, and he did not therefore speak decidedly from them. So far, however, they did not bear out Dr. Marshall Hall's plan. There was very little change produced in respiration, but more than one cubic inch—generally two or three. Dr. Sibson believed that emphysema did occasionally happen, and alluded to three cases of restoration of suspended animation, in one of which it had occurred. He then alluded to a case in which dislocation and fracture between the fourth and sixth cervical vertebræ had been produced by, he believed, zealously, but too roughly, carrying out Marshall Hall's method. The result of the injury was complete paralysis below the seat of injury.

Mr. SPENCER WELLS believed that insufflation from the mouth was the best method, and that it was best accomplished by passing a tube through the nostril into the glottis.

The PRESIDENT remarked on the importance of the subject, which made it a matter of great regret that such a wide difference of opinion should still prevail.—*Med. Times and Gaz.*, Feb. 2, 1861.

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14. *Palliative Treatment of Asthma.*—M. T. L. PRIDHAM, in an interesting paper on the treatment of asthma (*British Medical Journal*, Nov. 10, 17, and Dec. 22, 29, 1860), mentions the following as the most effectual palliative remedies which he has employed, though he states that no one of them is to be relied on in any second attack, for what succeeds to-day may fail to-morrow:—

"The first on the list is stramonium, the fumes of which may be collected in an inverted glass bowl with a narrow mouth; the bowl being charged to its full is placed under the mouth of the patient, who is directed to inhale to the fullest extent in his power the smoke which has been collected in the bowl, taking care to hold his head away from the bowl when an expiration takes place. Chloroform, both taken internally or inhaled, is a powerful remedy, but it must be employed with caution, and never administered except by a medical attendant.

The fumes of nitre paper in a state of ignition, well inhaled, is often a valuable remedy. Care should be taken to procure the best prepared from a good chemist. Chloric ether and the tincture of the lobelia inflata will occasionally relieve. Bicarbonate of soda, as well as chlorate of potass, given in full doses, I have frequently seen produce a good effect. Again, I have seen repeated doses of sulphate of alum procure relief, the powder being allowed to dissolve on the tongue before it is swallowed, in ten grain doses. I have also seen the fumes of tobacco, inhaled as I have recommended in the use of stramonium, relieve, when other remedies have failed; but I do not like this remedy, it produces such deadly faintness and nausea. Small drinks of the best Mocha coffee, made strong, will often procure relief. On two occasions, when every other remedy failed, I succeeded in procuring almost instant relief, by injecting two grains of morphia and a drachm of tincture of assafœtida. These were cases where mental distress appeared to be the exciting cause.

"I have often sat at the bedside of one, suffering from the severest form of the disease, watching with great anxiety the result of prescribed remedies, and it has not unfrequently happened that many have been tried without relief, the patient all this time gasping for life with sufferings the most intense, when relief has at length come from a remedy apparently the most unlikely to procure it—so capricious is the disease, and so uncertain the remedy in asthma cases of this particular character."

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15. *Sore Throat.*—Dr. Thomas Inman, in a recent paper (*British Medical Journal*, Jan. 26, 1861), maintains that the pain of sore throat and dysphagia is due to the muscles of deglutition being unusually irritable or inflamed, and in the treatment of these affections regard must be had to these muscles, as well as to the condition of the mucous membrane covering them. The new plan of treatment does not materially militate against the old, and the two may always be combined in one way or another.

He adds that the anodyne treatment is applicable to all forms of cynanche, whether it is catarrhal, scarlatinal, diphtheritic, syphilitic, or croupy; and that he has not yet found the laudanum act prejudicially in any case.

Acting on this idea, Dr. I. states that he has used opiate epithems externally for some time, and a few weeks ago began to use laudanum and water as a gargle. The effects have surpassed his expectations, and no case could more fully corroborate his views than the following: Mr. C., aged 30, after being much exposed to scarlatina, had ulcerated sore throat; this was so exquisitely painful, that he was unwilling to swallow anything; and he could not sleep, for he was constantly awakened by involuntary efforts of deglutition, which occurred as soon as he began to be oblivious; and he could not even speak intelligibly. Dr. I. used no internal application whatever but a gargle of laudanum and water. The patient reported the effect as magical, for he was able both to speak and swallow comfortably as soon as he had finished gargling; the ulceration remained, the *soreness* had gone. He had only to repeat the application three times in all. The only adjunct Dr. I. adopted was the use of iodine paint externally.

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16. *Perchloride of Iron in Diphtheria.*—M. Aubrun, in a communication to the *Académie des Sciences*, states that the greatest success has attended his administration of the perchloride of iron in diphtheria and croup. He gives the doses in rather a vague manner, stating that he adds from 20 to 40 drops of the solution of the perchloride—according to the severity of the disease and the age of the patient—to a glass of water, and causes the patient to take about two teaspoonfuls every five minutes during the day and every quarter of an hour during the night. Immediately after each dose of the perchloride some cold milk, without sugar, is given. This treatment must be scrupulously followed for some days, without regard to the patient's sleep for the first three days. It is not until after the third day that the false membranes begin to soften and separate. The author considers that from 140 to 350 drops of the solution, representing from 1½ to 4½ drachms of the perchloride should be taken during each 24 hours; and he carefully avoids giving any article of diet likely to decompose it.—*Union Med.*, No. 146.

17. *Severe Primitive Gangrenous Angina successfully treated by Perchloride of Iron.*—Dr. HENRY MUSSET relates a case of gangrenous disease of the back of the mouth which resisted all other remedies, but yielded to the employment of perchloride of iron. This medicine was administered in a draught at periodical intervals, and under its use the fever abated, the delirium ceased, and a large patch of ecchymosis which had appeared on the right side of the chest began to grow pale. Broth and wine were then administered, and every day the improvement continued; the breath was no longer fetid, the tissues became clean, and at length the patient, who had appeared moribund, was restored to life. Dr. Musset attributes the recovery entirely to the use of the perchloride, because it was the only medicine employed internally from the time when the worst symptoms appeared, and because the improvement continued regularly as soon as its administration was begun. With regard to the patch of ecchymosis which appeared on the chest, the perchloride seems to have acted beneficially in removing it, whether this interstitial hemorrhage was produced by the gangrene having destroyed some vein, or whether it was the consequence of an alteration of the fluids under the influence of a general cause. It is certain that under the influence of the perchloride of iron both the gangrenous disease of the throat and the ecchymosis on the chest were completely cured.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *L'Union Méd.*, Sept. 6, 1860.

18. *Alcohol as an Abortive Agent in Intermittent Fevers.*—Dr. JULES GUYOT proposed to treat the epidemic cholera in 1849 by the administration of alcohol, and the disease was often arrested by the adoption of this plan, which was carried out with still greater success in the epidemic of the same disease in 1854. As Dr. Guyot regards cholera as a species of fever, he proposes the same treatment in intermittent fevers, the alcohol being administered at the commencement of the paroxysm. Several cases are recorded in which this mode of treatment was successful. In one case, a man was attacked with a tertian fever, and two paroxysms were allowed to pass without treatment; but at the commencement of the third, just when the cold shivering was most developed, two small glasses of rum were administered to him with immediate relief; he became warm and felt better, and a third glass of rum was given. In half an hour afterwards the patient was dressed and walking out in the air, and the paroxysm never returned. Several other cases were treated in the same manner with equal success. In the case of a gentleman who had contracted an intermittent fever in Africa, and who came on a visit to Dr. Guyot, the paroxysm was arrested by the same means. Dr. Guyot's treatment of intermittent fever is explained in the advice given to this gentleman when consulting Dr. Guyot for his complaint. "If," said Dr. Guyot, "you have still eight hours before the commencement of the paroxysm, take twenty centigrammes of sulphate of quinine, together with a cup of warm infusion of chamomile, and in two hours repeat the dose. If you have only three hours, take three cups of strong coffee, and put your feet before a good warm fire. Lastly, if you have no time, or if you are not sure of the hour when the invasion of the paroxysm will happen, allow it to come on, and as soon as you are certain that it is fully developed, take two small glasses of rum, and five or ten minutes afterwards take a third." The last alternative was adopted, and with complete success.—*Brit. and For. Med.-Chir. Rev.*, Jan., 1861, from *L'Union Méd.*, Sept. 11, 1860.

19. *Topical Febrifuge.*—M. de Castelnau, editor of the *Moniteur des Sciences Méd. et Pharm.*, states in his No. for 23d Feb. 1861, that he has received from M. SÉZERIE an elaborate memoir, containing a great number of cases demonstrative of the efficacy of the following topical application in intermittent fevers of every type: ℞.—Essence of turpentine, 125 grammes; Sydenham's laudanum, 6 grammes; camphor, 3 grammes; olive oil, 60 grammes.—M. So soon as the apyrexia commences, the whole length of the spine is to be rubbed with this liniment for about ten minutes, and the application to be repeated every six hours until the next paroxysm. The next paroxysm is generally much slighter, and, after the third or fourth does not recur. It is well, however, as a matter of precaution, to repeat the frictions a few times after the cessation of the paroxysms.

20. *Apiol in Amenorrhœa and Dysmenorrhœa.*—By M. Joret. During the investigations into the antiperiodic power of apiol, carried on some years since by the author and M. Homolle, they had occasion to note its emmenagogue power, and subsequent trials have proved it to be not only one of the most active and most certain, but also a very harmless emmenagogue. In his preliminary observations, the author strongly insists upon the important precept that the treatment of amenorrhœa and dysmenorrhœa must be based upon an exact appreciation of the cause, thus: 1. When the cause is plethora, general or local bleeding, baths and emollients are the best emmenagogues. 2. When it depends upon a chloro-anæmic condition, whether spontaneous or accidental, martial preparations, good diet, a healthy abode, and exercise in the open air are the means most likely to act in restoring the menstrual flux. 3. When the disturbance of menstruation is connected with general nervous excitability, prolonged cold baths should predominate in the treatment. 4. But when the cause is a deficiency of general nervous stimulus, or of that localized in the genito-uterine apparatus, a perversion in the vitality of this apparatus, and especially of the ovarian sensibility, the stimulants termed emmenagogues are indicated; and it is in these cases, which are very numerous in practice, that apiol seems to possess decided superiority to any other means. 5. In those complex cases, in which several causes seem to combine in the production of the amenorrhœa or dysmenorrhœa, we should combat the various general or local conditions already adverted to before having recourse to emmenagogues.

Not only must we first investigate the nature of the cause of the amenorrhœa or dysmenorrhœa, but also determine the exact time at which the emmenagogue should be administered. This should be always at the period at which menstruation ought to return. There may be difficulty in exactly fixing this, especially when the menses have been long absent: but there are almost always symptoms arising from a congested state of the uterus which will sufficiently fix the period for commencing the remedy. The apiol may be given in capsules, each capsule containing four grains of apiol. Generally a capsule is given morning and evening, with a little sugared water. These are continued during the four or five days of the menstrual epoch. The month after, and even the third month, if menstruation has not become sufficiently abundant and quite regular, exactly the same procedure is to be observed. As a general rule, the menses appear more or less abundantly after the first course of doses, and it is rare for the medicine to be required to be taken after the third month. In dysmenorrhœa precisely the same course is to be observed. M. Joret quotes several cases exhibiting the decided efficacy of the apiol in primary amenorrhœa, or deficiency of secretion; in accidental amenorrhœa, or suppression, and in dysmenorrhœa.—*Med. Times and Gaz.*, Jan. 26, 1861, from *Bulletin de Thérap.*, tome lix.

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21. *Analysis of fifty-two Cases of Epilepsy.*—Dr. Edward H. Sieveking presented to the Royal Medical and Chirurgical Society (Feb. 12, 1861) a very interesting analysis of 52 cases of genuine epilepsy observed by himself. This was the second contribution of the same character as the one admitted into the *Transactions* of the society for 1857, and the author has limited the analysis to the same number of cases, taken in the order of observation, as had been subjected to analysis on the former occasion. Only those points were brought forward on which satisfactory evidence could be obtained. *Sex.*—23 were females, or 44.2 per cent.; 29 were males, or 55.8 per cent. Taking the two series together, the ratio of females to males was as 45.2 to 54.8 *Age.*—The following was the distribution throughout the different periods of life: Under 10 years, 12 cases; from 11 to 20, 25 cases; from 21 to 30, 7 cases; from 31 to 40, 3 cases; from 41 to 50, 2 cases; above 51, 3 cases. The basis of this calculation is the time at which the epilepsy first showed itself. Arranged according to sex, we find during the first decennium 8 males and 4 females; during the second, 11 males and 14 females; during the third, 5 males and 2 females; during the fourth, 3 males; during the fifth, 1 male and 1 female; during the sixth, 1 male and 2 females. *Causes.*—An hereditary taint was traceable in 14 cases, but in 8 only of these was there evidence of epilepsy having occurred in a near relative of the patient.

The exciting causes were traced in 37 cases as follows: uterine derangement, 9 cases; venereal excesses, including masturbation, 6 cases; fright, 4 cases; overwork, 4 cases; anxiety, 3 cases; dyspepsia, 3 cases; dentition, 2 cases; scarlet fever poison, 2 cases (doubtful); meningitis, anæmia, blood-poison, and the development of puberty, each 1 case. *Premonitory Symptoms.*—Comprising under the term "aura" all symptoms indicative of a near approach of a paroxysm, the author found it in 21 cases, or forty per cent., or less frequently than it was met with in the first series, where 52 per cent. of the cases exhibited premonitory signs. *Headache.*—Headache is very commonly associated with epilepsy, but its significance varies much according to its period of occurrence; it may be habitual, or it may be connected with the attacks only as a precursor of a sequela. It was constant or frequent in 9 cases, 17.3 per cent.; it occurred after the seizures only in 12 cases, or 23.0 per cent.; it occurred only immediately before or after the fits in 3 cases, or 5.7 per cent. *Biting the Tongue.*—Although a large number of genuine cases of epilepsy occur in which this symptom is never met with, it is important as a corroborative symptom. In the first series it was met with only in 32.7 per cent.; in the present, 28 patients, or 53.8 per cent., exhibited this feature. It did not appear that the female sex, as might have been anticipated, were less prone to inflicting this injury upon themselves than males; because of the 24 cases in which the tongue had not been bitten, 16 were males and 8 females. *Urine.*—The author has found no constant derangement in the urine associated with epilepsy. He has always failed to discover any sugar, nor has he met with a constant or even frequent excess of phosphates, or a diminution of urea. In several cases he found the urine presenting a specific gravity ranging about 1030, and containing a large excess of urea. There was in the present series no case of albuminuria, except, perhaps, in one instance, in which a small quantity of albumen appeared to be present for a brief period. An excess of phosphates was observed twice, oxalates in one or two cases, and an excessive deposit of lithates in a small number of cases. *Treatment.*—The author, while urging the value of treatment in alleviating the disease and indefinitely postponing the seizures in a large number of cases, admitted the doubt which always attached to any absolute cure of epilepsy. He advocated no specific, but the employment of all rational means indicated by the constitution and peculiar symptoms of each individual case. Above all, he considered perseverance in a combination of moral, regiminal, and medical treatment essential. As many of the cases analyzed had not been under the author's care permanently, he did not regard the results as to treatment indicative of what might have been effected in the whole series. He considered that he could lay claim to eight cures, that fifteen were decidedly benefited, while the remainder were either uninfluenced by treatment or did not continue under observation for a sufficient period to justify any positive statement as to the result.—*Med. Times and Gaz.*, Feb. 23, 1861.

22. *Embolia and Thrombosis.*—It is now nearly fourteen years since the theory of the detachment of fragments of vegetations from the valves of the heart, and from coagula in the large veins, and of the consequent secondary occlusion of the minute arteries, was first propounded by Virchow. The term *emboli* has been applied to the small plugs detached from the valves of the heart; *thrombi* to those derived from the veins. The effects produced in the different organs by these obstructions, and the entire doctrine of embolia and thrombosis, have of late been the objects of considerable investigation. A most elaborate work upon the subject has recently been published by Professor Cohn, of Berlin (*Klinik der embolischen Gefasskrankheiten mit besonderer Rücksicht auf die artzliche Praxis*, 8vo., Berlin, 1860 p. 696). Still more recently, an able memoir on the same subject, and a review of the entire doctrine of embolia have been published in the first number of *Schmidt's Jahrbücher* for the present year, by Dr. H. MEISSNER, of Leipzig. He considers the subject under the following heads:—

1. Obstructions of the cerebral arteries.
2. Obstructions of the veins and sinuses of the dura mater.
3. Obstructions of the aorta and arteries of the lower extremities.

4. Obstructions of the pulmonary arteries.

5. Obstructions of the gastro-epiploic, splenic, and other abdominal arteries.

Particular attention is devoted to emboli of the cerebral arteries, of which the author has collected 32 cases from various sources. From these cases, the following conclusions are drawn:—

1. Both sexes are equally liable to cerebral emboli. Of the 32 cases, 15 were females, and 17 males.

2. As to age, eleven of the cases were between 14 and 30 years of age; ten, between 30 and 40; five, between 40 and 50; and six, between 50 and 60.

3. The left half of the brain (16 cases) was more frequently affected than the right (11 cases). In 5 of the cases, the obstruction was in the middle line, and both sides of the brain were affected.

4. The most frequent site of the obstruction was in the cerebral carotid (nine times in one, and twice in both); the next, in the profunda cerebri or posterior cerebral artery (8 cases); and the next, in the artery of the fissure of Sylvius (7 cases). It was in the basilar artery in 4 cases; in the vertebral in 2 (in one of these, both vertebral arteries were blocked up); and in the arteries of the corpus callosum, in 2 cases.

5. Softening of the corresponding parts of the brain almost universally follows the obstruction. In 2 only of the cases, was there merely hyperæmia of the cerebral matter; and in 2 others no change whatever could be discovered.

6. In all but two cases, there was paralysis of the side of the body opposite to that of the obstruction.

7. Loss of consciousness during the attack was only observed twelve times.

8. The attacks were almost invariably sudden (30 cases). In 2 cases only had there been premonitory symptoms. Eight times, repeated attacks of paralysis had occurred. In one case only was the commencement of the paralysis gradual; while, in one other, its mode of commencement was unknown.

9. The heart was diseased in 25 cases. In 22 cases there was disease of its walls or valves; in 2 there was merely hypertrophy; and in one case there was an aneurism of the ascending aorta. In 7 cases, nothing abnormal could be detected in the heart.

10. In 15 cases there were obstructions of vessels in other parts of the body, or infarctions.

11. Among the coexisting diseases, phthisis, syphilis, and alcoholism were the most frequent.

From these cases, collected by Dr. Meissner, it is obvious, that the so-called emboli of the cerebral arteries cannot always be derived from vegetations on the valves of the heart. Cases are quoted from Bristowe, Jenner, Tüngel, and Hasse, where there were all the ordinary appearances of emboli of the cerebral arteries, but where the heart was perfectly healthy, and where no other source could be discovered in the body, from which clots could have been detached. In such cases, the lesion must be attributed to a local coagulation or spontaneous arteritis.

In the case of the pulmonary arteries, the plugs may be derived from coagula in the larger veins, or the coagulation may be of a local character, and result from a morbid condition of the blood, from dilatation of the right side of the heart, a firm fatty degeneration of its muscular tissue. Either of the two last mentioned causes will impede the progress of the blood through the lungs, and so favour its coagulation and the development of obstructions.—*British Med. Journ.*, Feb. 2, 1861, from *Schmidt's Jahrbücher*, 1861, No. 1.

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23. *Remarkable Case of Hydrophobia.*—Dr. W. R. Gore relates (*Dublin Med. Press*, Jan. 9, 1861) the following remarkable case:—

"Some time since I was called to see a man who was reported to be in a state of 'great excitement, labouring under fits of violent screeching.' On entering the room where he was, I found a large number of persons, but not near the bed. The man's wife was endeavouring to hold a stout middle-aged person, who took a terrific fit of screeching, as I entered the room. His aspect was wild, and he grasped violently at the wife when the fit came on. Having got rid of the visitors, I commenced to explore the case, during which I was repeatedly inter-

rupted by the violence of the fit and the intensity of the screeching. In the intervals he was calm and rational. The *cause* of the fit he attributed to an excruciating agony which, at each time, suddenly seized upon his *penis*, throwing him into the excitement described. He declared he had no other disease, and that if this was not cured he must soon die, 'he could not stand the pain.' I examined the parts, but they were perfectly healthy. During the process he had two attacks, but they produced no physical alteration in the organ. During one of these he attempted to seize and bite me, but I escaped his grasp. He made no such attempt upon the wife. I was puzzled to account for these strange symptoms, there being no other to give me a clue to the cause of them at the moment. I recollected the countenance of the man, but he made no reference to any previous acquaintance with me. Having called in some of his male friends to learn somewhat of his history, one asked me if I did not remember the man who had been some fourteen months previously under my care in the infirmary with a wound of the leg from a bite of a mad dog. The case was soon revealed. Returning one evening from the Messrs. Fitt's brewery, where he was employed, he was seized in Peter Street by a dog said to be mad. He was brought to the infirmary, where the part was burned well with caustic, and though he remained there for two months it did not heal sufficiently to permit him to work. He spent five or six months in the workhouse hospital with similar results; but the sore became so chronic he returned to his usual avocation, carting with an ass and car. He daily continued this avocation for six or seven months without any sickness, save a chronic ulcer of the left leg, which he regularly dressed with ointment.

"Early on the morning of the day I saw him, he was suddenly and unexpectedly seized with a fit such as I have described. He refused all nourishment. I visited him about two P. M. Having made myself master of these facts, I proceeded to test the case. Every time I approached him with a drink, or that he saw the bowl of water, he got a violent spasm, screeched, thrust his hands to seize the penis, and shivered, with contraction of the mouth and jaws. The Rev. Mr. Fitzgerald, C.C., having entered the room at this time, I held with him a consultation as to the best and safest course to be adopted to insure safety and prevent terror and annoyance to other patients. We agreed that a lunatic ward in the workhouse was the proper place for his treatment under the circumstances. The clergyman sent for a covered car, and wrote to the excellent physician of that establishment, Dr. Brodie, under whose care he was soon placed. Notwithstanding the most energetic treatment, scarcely twenty-four hours elapsed before death put an end to his sufferings. Incessant spitting, extreme terror, violent convulsions, wild delirium, were succeeded by sudden prostration and death. The sore on his leg never healed. He had no recognizable premonitory stage, none of the pharyngeal spasm, except when attempting to drink, and the characteristic anguish was transferred by some sort of reflex action to the genitals with electric intensity. These peculiarities render the case remarkable and worthy of record. The bite was a severe one, the parts much torn, nitrate of silver and the knife the first remedies employed, every effort completely to cicatrize the sore failed, and the issue took fourteen months or so to smoulder before developing itself in a rapid and tragical death."

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24. *On the Causes of True Leprosy.*—Mr. W. E. C. NOURSE read a paper on this subject before the Royal Medico-Chirurgical Society (Feb. 12, 1861). The author, having seen this disease at Bergen and elsewhere, first adverted to the well-known fact that leprosy was a blood disease, or dyscrasia; that an altered condition of the blood, demonstrable by chemical analysis, accompanied the premonitory symptoms which preceded every fresh appearance of leprous blotches, both in the tubercular and in the anæsthetic form of leprosy, the blood at such times containing excess of albumen and fibrin; and that when the blotches were fully developed, the blood was found to be restored to its usual condition. Since, then, a changed condition of the blood was the proximate cause of leprosy, it was natural to look to the well-known agents concerned in making blood for the next link in the chain of causes of the disease. And here there was abundant evidence. Leprosy was frequent in Iceland, where the people inhabited small,

close, dimly-lighted huts; were dirty in their personal habits; breathed a moist marine atmosphere; often had their garments wet; and lived on inferior food, badly cured. In Norway, where the food was inferior and too long kept, leprosy occurred; and on part of the west coast, where the food was unusually bad—where the people inhabited small, dirty, unventilated huts in damp and cold situations, and often had their garments drenched with rain or salt water—leprosy was peculiarly frequent. During the middle ages, when population had become too numerous to be sustained by the produce of the chase, and before the art of producing large supplies of good food was understood, leprosy had been common in all parts of Europe; and it had disappeared precisely as more copious and better supplies of fresh meat, vegetables, and bread were produced. It had then vanished first from England, then from Ireland and Scotland, and more recently from the Isles. On the Continent it had followed a similar rule, and the disease now only remained in particular maritime situations, where the standard of living was miserably low, the habits of the people dirty, and their dwellings small, dark, and ill-ventilated. The author believed that these conditions would always be found to coexist with it, and that it would be endemic wherever they obtained, irrespective of latitude. In Egypt, cases of leprosy were occasionally seen; and some of the Egyptians lived in a miserable squalid manner, and in small dirty huts. In Palestine, lepers were still found among the very poor, distinguished by want and dirt. Dr. Webster's account of the leprosy in Spain was precisely to the same effect, and he had shown that it prevailed in damp maritime districts, among people living in a poor miserable way, on bad and decaying food, and in close dirty huts. Mr. Nourse dwelt on these circumstances as the main causes originating the disease, and concluded by expressing his belief in its hereditary transmission.—*Med. Times and Gazette*, Feb. 23, 1861.

25. *Hypochondriacal Insanity as a Precursor of General Paralysis.* By Dr. Baillarger, of the Salpêtrière, Paris.—General paralysis is a common and most serious phase of mental disease. It attacks patients of all ages, and its progress towards a fatal termination exhibits stages of the most melancholy and humiliating nature. All medical men accord it as being insidious in its approach. It may be long in becoming fully developed, presenting at first only the most trivial indications, in many cases so trifling as to pass altogether unobserved; and when the malady does at last attract attention, it may be too late for arresting its advance. It is therefore most important to attend to this disease at the very first; and it is with this object that it seems useful to describe the intimate relation existing between the hypochondriacal form of melancholia and general paralysis.

This relation being understood, it becomes one means of detecting the advent of that disease at the very commencement of its attack. It is of importance to distinguish this symptom, as the melancholy accompanying general paralysis very much resembles melancholia in its simple form. The conceptions or illusions of the hypochondriac, however, although of considerable variety, are yet of such a tendency as often to present something of a special character in their nature. The patients believe that their various organs are changed, destroyed, or completely obstructed; they pretend, for example, to have no mouth, no abdomen, no blood—that their gullet is stopped up, their stomach quite full, their bowels shut up; they imagine that their food passes from its ordinary channels—that it gets into their skin, or even their clothes. Four patients believed their body to have become putrid. Many among such are afflicted with hallucinations of smells. Some keep their eyes closed, and allege they are blind; others cease to speak, and state afterwards that it was impossible for them to open their mouth; they assert that they cannot swallow, nor defecate, nor make water; they affirm that their members are altered—that they are larger or smaller; they say they do not exist, or even go so far as to believe themselves dead; they remain motionless, the eyes shut, and when their limbs are lifted they let them fall, as if completely paralyzed. These different delusions lead to serious consequences; many of the patients refuse, more or less obstinately, to

take food, and sometimes it becomes necessary to feed them by means of the stomach-pump; and such patients speedily become much emaciated. I have seen, says M. Baillarger, a lunatic die in eight days from obstinately resisting the employment of the stomach-pump, under the impression that his stomach was completely full, and his gullet obstructed. One patient pretended he could not make water, and used every effort to retain it; his bladder became enormously distended, and he was at last attacked by a veritable retention, and it was with great difficulty the catheter could be used. In the end a false passage was made, and the patient died, while yet in the first period of the disease.

The tendency to gangrene, which is one of the characteristics of general paralysis in its latter stages, exists in these cases markedly, and before its usual period. Four cases had large eschars over the sacrum, without ever having been confined to bed; one woman, who for a year had exhibited all the symptoms of commencing general paralysis, preserved every appearance of health otherwise, when, all of a sudden, she became affected with hypochondriacal melancholia, and six weeks afterwards died of gangrene in both feet.

Hypochondriacal delirium is thus not only a mere premonitory symptom of certain forms of paralysis, but it is a serious symptom, and one very unfavourable, in prognosis.

In reference to this affection—viz., that of hypochondriacal insanity—viewed as one of the precursors of general paralysis, this being the fact of most practical value in connection with it, the delusions of which we have spoken seem to claim especial attention, as they are sometimes to be detected in patients as yet evincing no indication of paralysis—this supervening at a later period. Such a termination is certainly not invariable; but it is so common after this symptom, and the prognosis in such cases is so unfavourable, that considerable importance seems to attach to the subject. Thus Dr. Combes published some remarks on a case of "Lypémanie," with stupor, and other serious symptoms—nothing, however, indicating that at a later period this patient should be attacked with paralysis; and, after fifteen months' residence in this asylum, where he was treated, he was dismissed as cured. In reading Dr. Combes' remarks, I was struck, observes M. Baillarger, with certain of the delusions affecting this patient. He had believed that he was about to die, if indeed not already dead; that his limbs were atrophied, that he had none, etc. These appearing to be good grounds for suspicion, I wrote Dr. Combes to know what had become of the patient. The answer confirmed my suspicions to be correct—the result having been that, after a year's return to his occupations, he had been attacked with general paralysis. We may see by this example, that, had hypochondriacal delirium been held as a certain precursor of general paralysis, this affection might have been foretold two years before it actually took place.

It may appear strange that one form of insanity should thus be urged as premonitory of paralysis. Singular as it may seem, however, it is not the first time that such a doctrine has been urged. Since the writings of Bayle, no medical man doubts the fact of certain forms of insanity, such as the ambitious form, being symptomatic of approaching paralysis. And if one form of delirium be held, in mania or monomania, as indicating the advent of paralysis, there seems no reason why this particular hypochondriacal form should not serve the same purpose, and with equal certainty, in melancholia.

We do not attempt explaining these facts; and we may add, it seems useless to do so, either here or in the case of ambitious insanity. One point connected with the ambitious form may be mentioned; and that is, the relative frequency of general paralysis among females in different ranks of society. While this malady is equally common among males of all classes, among women it is not so. It is very common among the poor, and very rare among the rich. It would appear, however, that this circumstance has been forgotten by those who would explain the greater number of cases of ambitious insanity as induced by ideas of speculation—by the desire of suddenly arriving at honours and fortune.

In conclusion, it appears evident that hypochondriacal no less than ambitious insanity may, in different circumstances, be considered as a prognostic of general paralysis. The intention of the present paper has accordingly been to direct attention more particularly to the latter of these forms. As for the first, I have

frequently had occasion, before now, to refer to it in all its remarkable psychological characters.—*Ed. Med. Journ.*, Dec. 1860, from *Gazette des Hôpitaux*, Sept. 1860.

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26. *Perversion of the Mental and Bodily Faculties as Premonitory Symptoms of General Paralysis.* By Dr. Brierre de Boismont.—This paper, read at the meeting of the "*Académie des Sciences*," Sept. 24, 1860, may be considered as a sequel to that of M. Baillarger, read at its previous meeting, and just noticed. After alluding to the medico-legal aspects of such cases as are characterized by a tendency to theft and other criminal propensities, the author states that his observations have been carefully collected from a hundred examples falling under his own care, and respecting which he had already communicated to the *Société Médico-psychologique* those alterations in character and disposition throwing light upon the question under discussion.

According to his observation, the most frequent symptoms—in fact, what occur in three-fourths of such cases—are, great irritability, movements of impatience, anger, and violence. In a much smaller proportion of cases, the disease assumes, on the other hand, characters of indolence, and apathy, and gentleness. Such patients are reasonable and well-conducted; but between their words and actions there is an insurmountable discrepancy observable.

In place of either of these states of mind, or along with either of them, we frequently find perversion of the moral faculties; individuals who until then had been of unimpeachable character, suddenly becoming irreligious, immoral, or dishonest. These indications are important to be observed, as they are often unsuspected by any one as being connected with the diseased condition existing. The propensity to steal is perhaps, of all others, the commonest among this class of symptoms, and seems to some extent connected with that peculiar flow of spirits common in such patients, and evinced in their delusion that they are rich, powerful, and lords of all that they see—a state of mind which, in its results, sometimes entails the most painful consequences.

It is thus certain that the greatest change in the character and conduct is often observed in connection with general paralysis, giving rise to acts of an eccentric or reprehensible nature. Such acts are, no doubt, frequently to be met with as ordinary manifestations of the disposition, but their sudden and unaccountable accession results from mental disease, and is especially connected with general paralysis; they are its premonitory symptoms—*avant-courriers*—as they have been styled by Dr. Forbes Winslow in his work *On Obscure Diseases of the Mind and Brain.*

Our principal reliance in the diagnosis of such cases must rest on the general bearings of the disease. In the greatest number of instances where sudden alteration in character, disposition, and conduct, become apparent, there is reason to fear the accession of general paralysis; if the age is from 35 to 45 years, and excess of some kind, such as sexual or intellectual excess, and hereditary predisposition can be added, the prognosis becomes all the more certain.

Besides these characteristic symptoms, we must not lose sight of a very common occurrence connected with them—that is, the frequency of attacks of the nature of cerebral congestion. This may occur in the shape of a transient stunning sensation or giddiness, and pass off without attracting much notice; but it does so more commonly, and is of serious importance. Such congestions entail weakening of the intellectual faculties, loss of memory, and absence of mind. The mind loses its precision, its ordinary power. If the patient resume his occupation, and take to any work requiring application, the difference is at once observable in his capability of conducting it. His benevolence is greater than usual; there is a confidence betraying itself in his speech, which foretells the advent of insanity in its ambitious form. On the other hand, but less frequently, there is a state of dejection, the tendency to melancholia and hypochondriasis.

But the disorders of the muscular system are the key or touchstone for our guidance in this disease. One of these is of much importance, and manifests itself in a passing, transient trembling of the lips; a scarcely perceptible embarrassment in speech; a hesitation in pronouncing a certain word or letter, which

sometimes does not occur except at long intervals. Taken by itself, this symptom may not be determinative, although it is of great assistance; but if it be added to diminution of motive power, such as may be observed in asking the patient to grasp one's hand, or his own limb, the certitude becomes increased tenfold. To these symptoms may be added, inequality of the pupils, exaltation or failure of the sexual functions, diminution of the cutaneous sensibility, tremors of the muscles, and the results of analysis of the urine, and the indications afforded by means of electricity. We have also, in many cases, adds the author, observed paralysis of the sixth pair, amaurosis, and attacks of deafness, precede by several years the occurrence of general paralysis, and serve as the means for its prognosis.—*Ibid.*, Dec. 1860, from *Gazette des Hôpitaux*, Oct. 1860.

27. *On the Communication of Syphilis by Vaccination.*—M. VIENNOIS, of Lyons, published, last year, an interesting paper on this subject in the *Archives Gén. de Méd.* The following are his conclusions:—

1. Syphilis has in many instances been observed to follow vaccination, ever since the introduction of that operation, and by authors worthy of credit, French, English, German, Italian, &c.

2. When a syphilitic subject is vaccinated, in whom the disease is in a latent state, syphilitic symptoms may be developed by the vaccine influence; these symptoms often consist in general eruptions of a papular, vesicular, or pustular character, but a chancre never forms at the seat of the vaccinal puncture.

3. On the contrary, if a healthy subject be vaccinated with vaccine virus taken from a syphilitic subject, and the lancet be charged at the same time with a little blood, as well as vaccine matter, the two diseases may be conveyed by the same puncture—the vaccine with the vaccine matter, and syphilis with the syphilitic blood.

4. In these cases, of which a number are on record, vaccination is first developed because its period of incubation is shortest, and its evolution less rapid than that of syphilis. The latter appears subsequently, and manifests itself by its characteristic lesion at the inoculated spot.

5. The initial lesion, then, by which syphilis, following the vaccinal pustule, manifests itself, is an indurated ulcer, with adenitis; in a word, all the phenomena of primitive syphilitic chancre. The great law announced by M. Rollet, that syphilis always commences by a chancre, even when it results from secondary symptoms, or even from syphilitic blood, is thus fully confirmed.

6. After the primary chancre is developed at the inoculated spot, and in the usual period, secondary syphilis occurs, and runs the usual course, as if transmitted in any other way.

7. When the mixture of virus does not take place accidentally, but is effected intentionally (as practised by MM. Spéreno and Daumés, by mixing the vaccine matter with the pus of chancre), the result is the same; one virus does not destroy the other, but each runs its separate course.

8. The vaccine matter thus acts as a simple vehicle for the virus contained in the syphilitic blood, which it divides and dilutes, as a drop of water would do, without at all modifying its properties or its effects.

9. It is important, then, never to take the vaccine virus from a suspected person, or from an infant whose parents are unknown before the age at which hereditary syphilis usually manifests itself.

10. If circumstances make this last necessary, great care should be taken to collect only the vaccine matter, free from blood or any other syphilitic humour.

11. In no case should a healthy subject be vaccinated with matter taken from a syphilitic subject, for, in spite of all precautions, there can be no certainty as to the purity of the vaccine matter.

12. These precautions are the more important, because, with the matter from one syphilitic subject a number of persons may be vaccinated, and syphilis conveyed to nearly all (as seen by Ceriale, of Cremona).

13. It is sufficient to point out these precautions, to avoid new evils, and to remove the cavils of the enemies of vaccination; for, in these cases, the propagation of syphilis is not the fault of vaccination, but of the vaccinator.—*Gaz. Méd. de Paris*, Jan. 26, 1861.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

28. *Contributions to the Subject of Compound Fracture; being an Analysis of 302 Cases.*—Mr. Thomas Bryant read a paper on this subject before the Royal Medical and Chirurgical Society (Feb. 12, 1861). Having recognized the fact that the science of medicine and surgery was one purely of observation, and that its principles and practice could only be regarded as sound so long as they were based on facts, the result of recorded and carefully-observed phenomena, the author went on to state that he believed it to be the duty of every practitioner to communicate to his professional brethren any positive material he may possess, and thus to add his quota in building up the noble art and science to which we have the honour to belong. In conformity with such a principle he had the pleasure of presenting to the society a brief analysis of upwards of 300 cases of compound fracture of the extremities, omitting only those minor examples in which the smaller bones of the hands and feet were involved, believing that the results educed were not unfruitful either of interest or instruction. The cases were all taken from the experience of Guy's Hospital within the last twenty years the earlier ones having been collected from the records of the institution, and the author's own notes having furnished him with the particulars of every case admitted within its walls during the last seven years. He then proceeded to the analysis of the whole number—302 cases:—

17 or	5.6 per cent.	were of	the thigh
193 or	63.9 "	"	leg
35 or	11.5 "	"	arm
57 or	18.8 "	"	forearm

11 out of the 17 cases of compound fracture of the thigh proved fatal, or 64.7 per cent. 74 out of 193 examples of compound fracture of the leg were fatal, or 38.3 per cent. 4 out of the 35 cases of compound fracture of the arm died, or 11.4 per cent. And 7, or 11.2 per cent. of the 57 cases of compound fracture of the forearm. Of the whole number of 302 cases, 96 were fatal, or 31.7 per cent. A table then followed showing the causes of death in these 96 cases, from which it appeared that in cases subjected to amputation pyæmia was twice as fatal as in others treated on conservative principles, and that exhaustion and gangrene were more common causes of death; that in cases treated on ordinary surgical conservative principles delirium tremens and tetanus were more common causes of a fatal result. Analyzing the 17 examples of compound fracture of the thigh —9 underwent primary amputation; 6 died, and 3 recovered. 1 underwent secondary amputation; fatal. 7 were treated on conservative principles; 4 died, and 3 recovered. 10 were thus treated by amputation, and 7 of these died, or 70 per cent. 7 were left to nature's efforts for repair, and of these 4 died, or 57 per cent. Having given an outline of the fatal cases, the author went on to remark, first, on the rarity of compound fracture of the thigh, the accident bearing the proportion only of 5.6 per cent. to the other cases involving the larger bones of the extremities. This fact, he stated, was well borne out by the experience of all surgeons. The excessive mortality of these cases was the second point to which he alluded, 64 per cent. of the whole number of cases proving fatal. In nearly 60 per cent. amputation was had recourse to, and 70 per cent. of these cases subsequently sank. The extreme severity of the majority of these cases, he remarked, rendered primary amputation absolutely essential. He showed that more than half the examples died, or 57 per cent., in which attempts had been made to save the limb; and that the cases in which recovery had taken place were in young and healthy subjects. The author passed on to review the opinions of some military surgeons on the subject, and pointed out how Dupuytren, Hennen, Larrey, Guthrie, and others all agreed that in compound fracture of the thigh from gunshot wounds, "in rejecting amputation, we lose more lives than we save limbs;" and also "that in the exceptional cases, which result in consolidation, the condition of the limb is not encouraging." He quoted Macleod's Crimean experience as indicating the same opinion, this surgeon advising

amputation in all such cases when taking place in the middle and lower third of the thigh, and hesitating only in a like recommendation in cases of fracture of the upper third, on account of the extreme mortality of such amputations. Malgaigne's and Bauden's experience was then quoted to prove the poor success of conservative treatment in these injuries, the latter surgeon saving only 2 out of 25 cases, and these two retained useless and deformed limbs. The author then went on to say that the experience of civil surgeons was not unlike that of their military brethren, although it was much more limited; and expressed an opinion, which he believed to be generally entertained, that a satisfactory result can rarely be obtained by conservative treatment, except in the most favourable cases—that is, when the subjects are young and the fracture uncomplicated; when the soft parts are not materially damaged nor the bone comminuted. When the bone was comminuted and the soft parts seriously involved (such a condition being generally produced by local mechanical violence), he believed that a satisfactory termination of the case must be regarded with doubt. If the patient should be old or unhealthy, amputation should be at once performed; and if there should be a doubt as to which line of treatment to adopt, the safest is to decide on amputation. But if the subject should be a young and healthy one, and not subjected to injurious hygienic influences, the author believed that a satisfactory termination might be obtained by removing at once the broken and disconnected fragments of bone (enlarging the wound if necessary), and by maintaining the absolute repose which is so essential. He stated that he believed it to be bad practice to leave the broken fragments in position, with the hope of union; and that by doing so we left a constant source of irritation, which must retard the local process of repair, as well as seriously weaken the powers of the patient, a subsequent operation being almost necessarily required to remove what will become necrosed bone.—*Med. Times and Gaz.*, Feb. 23, 1861.

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29. *Complicated Fracture of the Leg, followed by the Formation of a False Articulation; Cure effected by the use of a Seton.*—M. JOBERT DE LAMBALLE, at a late meeting of the Imperial Academy of Sciences, narrated the following case, which is of some value in both its physiological and pathological relations:—

A gentleman, forty-two years of age, was thrown from his carriage on the 30th of June. A severe direct fracture of the right leg was the result of this accident. Various applications were employed, including plaster, and starch bandages, but at the close of about three months it was ascertained that union of the ends of the fracture had not taken place. For two months subsequently the limb was put in an ingenious apparatus devised by M. Seution, which admitted of free motion of the limb whilst the fracture was kept immovable, an opening being left in front, so as to allow of its being easily painted with tincture of iodine.

At the expiration of this period no change had occurred in the condition of the patient, and he was desirous of submitting to an operation with a view to recovering the lost functions of the limb. A seton was accordingly introduced between the opposed ends of the fracture, the thread being brought into contact with the periosteum without touching the bony extremities; it was kept in position for ten days, and the suppuration became exhausted six days after its removal.

This operation was performed on the 17th of January; on the 12th of February the mobility between the fragments had become obscured, and at the end of the same month there was no longer any trace of it; and the patient was soon after able to walk.

Evidently, there was no exfoliation and no necrosis, in consequence of the application of the seton, so that the callus must have been necessarily formed under the influence of the excitation of the periosteum, an evident source, in similar circumstances, of all bony cicatrization, as has been demonstrated by M. Flourens.

The question is, consequently, not concerning a secondary callus resulting from the formation of granulations occurring after a superficial necrosis of the fragments, but rather of a cicatrix which has for its origin the deposit of a pro-

duct furnished by the enveloping membrane of the bones upon which has devolved the property of reproducing and regenerating them.

The duration of treatment was very different from, being much shorter than, that of patients in whom M. Jobert applied the seton by placing it next the surfaces of the extremities of the fragments, and this will be readily understood if we take into consideration the fact that the periosteum alone furnished the means of cicatrization, whilst in the other cases there were necrosis and granulation of the ends of the bone.—*London Med. Review*, Sept., 1860.

30. *Digital Compression in Aneurism.*—M. Mirault, of Angers, related to the Paris Surgical Society the following two cases of aneurism, in which digital compression had been successfully employed:—

1. A man, aged 23, exhibited an aneurismal tumour at the bend of the arm, some time after being bled. When admitted into the hospital this tumour was about the size of half an egg. Digital compression was made on the brachial artery, at about the middle of its course, from 11 o'clock A. M. to 9 P. M., and next day it was resumed from 6 A. M. to 9 P. M. The tumour became more firm, and the compression was employed again at 6 A. M. of the third day; at 8 the pulsations had become indistinct, and at 12 they had completely disappeared. Thirty-one hours altogether had been occupied in making compression.

2. A child, aged 9, having had the trunk of the temporal artery opened, just anterior to the ear, an aneurism about the size of a nut appeared eight or ten days after the accident. Direct digital compression was employed for five hours on the first day; for ten and a half hours on the second; for eleven hours on the third; for nine and a half hours on the fourth; for eleven hours on the fifth; for ten hours on the sixth; for ten and a half hours on the seventh; and for nine and a half hours on the eighth—making a total of eighty-five hours, at the end of which time the aneurism had become cured.—*Med. Times and Gaz.*, Jan. 19, 1861, from *L'Union Méd.*, No. 1, 1861.

31. *Arrest of Venous Hemorrhage.*—In the course of an interesting paper by Prof. Langenbeck on the "Surgical Pathology of Veins," illustrated by numerous cases, he observes that styptics are not suitable for the arrest of venous hemorrhage. The best of these, the liquor ferri sesquichlorati, is dangerous, owing to the extensive thrombus formations and subsequent irritating effects it gives rise to. In all cases, when obstinate venous bleeding proceeds from several small veins, he gives decided preference to the actual cautery, as most certainly guarding against the breaking up of thrombi and pyæmia. When the bleeding proceeds from a large vessel, compression, ligature of the vein, or ligature of the corresponding artery, should be resorted to. In wounds of the large veins of the extremities, *compression* of the peripheric end by means of the finger will usually suffice; and in wounds of the jugulars, we should at once apply the finger to the central end to prevent the entrance of air, and then to the peripheric end to arrest the bleeding. In the case of a large wound of the jugular, the finger can only act provisionally, and the best means of proceeding consists in closing the lips of the outward wound by strips of plaster (which must not extend to the uninjured side of the neck, where they would compress the opposite jugular), so applied as to exert the most equable compression around the wound without impeding the circulation. In the case of the veins of the extremities, bandages may also be exactly applied, commencing at the toes or fingers. When the injured vein is at the bottom of a wound, the author places some cerated linen in contact with it, fills the wound with charpie, and then brings its edges together with plaster.

Ligature of the Vein.—In general, tying the peripheric end of a wounded vein of the extremities suffices; but a ligature both above and below the wound may be required when a considerable branch enters just above the central end. To avoid the loss of blood during the removal of large tumours, the provisional ligature of several large subcutaneous veins, which sometimes acquire the size of the finger, may be requisite; and in such cases the author always applies a double ligature, and divides the vein between, removing the ligatures after the

completion of the operation. This practice is the more to be recommended from our ignorance of the conditions under which air gains entrance by dilated subcutaneous veins. In wounds of the external jugular, the central end should always be tied, as it should be prior to operations likely to lead to its being opened. Under other circumstances, the ligature of large veins should be avoided as much as possible, especially in hospitals, where the danger from thrombosis and pyæmia is increased. The internal coat of a large vein is not divided by the ligature, as in the case of an artery; and the inner walls approximated by the ligature may unite before the separation of the latter—the vessel remaining pervious, though somewhat narrower, up to the very seat of the ligature. When inflammation follows a wound or a ligature, more or less extensive thrombosis may succeed, which may lead to detachment of coagula or pyæmia. When this is not the case, the vein gradually becomes pervious again; and so great is the regenerative capacity of veins, that, even when large portions have been removed, these may be reproduced, effecting a junction between the separated ends of the still pervious vessel.

Ligature of the Artery.—It is obvious, *à priori*, that compression or ligature of the corresponding arterial trunk, by preventing the access of blood, must arrest hemorrhage from a large vein; but it does not appear that any one has yet practised the ligature for such a purpose. One reason of this seems to be derived from the fact that when compression of an arterial trunk, as the carotid, subclavian, or femoral, has been made, through the soft parts, at some point between the heart and the bleeding wound, the bleeding from the jugular, axillary, or femoral vein has not immediately ceased. This is partly because the peripheric veins still continue to pour their blood into the injured vein, and partly because an effectual compression of the carotid or femoral artery is not possible without accompanying compression of the jugular or femoral vein. Compression of the arterial trunk, therefore, not having been attended with the same immediate effect as in arterial hemorrhage, it has been believed that no good result was to be expected from the ligature. Again, it has continued, until the most recent times, a cherished opinion among most surgeons, that the simultaneous tying of a large venous and arterial trunk must give rise to gangrene—a fear which experience has shown to be unjustifiable. In fact, when both artery and vein are tied, not only does no gangrene follow, but there is less disturbance of the capillary circulation than when only one of these vessels is submitted to the ligature. In two cases related by the author in which the carotid and common jugular were both tied, no disturbance whatever of the cerebral circulation took place, and neither patient exhibited any of the symptoms which have been met with when ligature of the carotid alone has been practised. During the establishment of the collateral circulation an equilibrium between the arteries and veins has been maintained. In this ligature of the artery, then, we have a safe means of treating venous hemorrhages which may otherwise prove fatal, and the author relates a case in which he made application of it. During the removal of a large sarcomatous tumour from the thigh, the much-enlarged and brittle femoral vein was opened. The hemorrhage was excessive, repeatedly resisting every attempt to arrest it, and the patient was well-nigh lost. The femoral artery, already exposed during the operation, was tied, and the bleeding at once ceased. The ligatures which had been passed around the vein were removed, and the wound dressed. The patient did well. This, as far as the author is aware, is the only case in which an arterial trunk has been intentionally tied for the arrest of a dangerous venous hemorrhage. He refers, however, to cases quoted by Dr. Crisp, in which bleeding from wounds of the vein made during the operation for popliteal aneurism ceased after the application of the ligature to the artery. The author recommends that as soon as compression proves without avail in hemorrhage from large venous trunks that the artery should be at once tied—the simultaneous tying of the injured vein being unnecessary and unadvisable from the danger of thrombosis it gives rise to.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Archiv für Klinische Chirurgie.*

32. *Ovariotomy in London Hospitals.*—The following table, taken from a late number of the *British Medical Journal* (Dec. 1860), embraces, it is said,

all the operations performed during the last three years in the London hospitals, except those in the practice of Mr. Baker Brown at the "London Home."

The operations are numbered in the order of their performance.

No.	Hospital.	Operator.	Date of operation.	Result.
1	Samaritan	Mr. S. Wells	Feb. 1858	Recovered
2	Metropolitan Free	Mr. Hutchinson	Aug. "	Do.
3	Ditto	Ditto	Aug. "	Died
4	Ditto	Ditto	Sept. "	Do.
5	Samaritan	Mr. S. Wells	Sept. "	Recovered
6	Metropolitan Free	Mr. B. Childs	Nov. "	Died
7	Samaritan	Mr. S. Wells	Nov. "	Recovered
8	University College	Mr. Erichsen	Nov. "	Died
9	Samaritan	Mr. S. Wells	Jan. 1859	Do.
10	Guy's	Mr. C. Forster	Feb. "	Do.
11	Metropolitan Free	Mr. B. Childs	Feb. "	Do.
12	Samaritan	Mr. S. Wells	May "	Recovered
13	Ditto	Ditto	June "	Died
14	Ditto	Ditto	June "	Recovered
15	Ditto	Ditto	July "	Do.
16	Ditto	Ditto	Oct. "	Do.
17	Westminster	Mr. Holt	Oct. "	Died
18	Samaritan	Mr. S. Wells	Dec. "	Do.
19	Ditto	Ditto	Jan. 1860	Recovered
20	London	Mr. Curling	Feb. "	Died
21	Samaritan	Mr. S. Wells	Feb. "	Do.
22	Middlesex	Mr. Nunn	Oct. "	Do.

The result, it will be perceived, is 13 deaths and 9 recoveries. Two of the latter, however, after recovering from the operation, died some months afterwards of cancer.

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33. *Ovariotomy in Canada.*—Dr. Reginald Henwood, of Brantford, C. W., records a case of ovarian tumour in an unmarried woman, 38 years of age, in whom he removed the tumour, through an incision extending from two inches above the umbilicus to within an inch of the pubis. No anæsthesia was used. The tumour proved to be the right ovary, and weighed 7½ pounds, twenty-four hours after removal, when it must have lost a considerable portion of its weight. The wound healed by the first intention. In seven weeks the patient was able to go home.—*British Am. Journ.*, Dec. 1860.

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34. *Extirpation of the entire Parotid.*—M. Marzolo relates (*Gazetta Medica Italiana*) a case of a woman 50 years of age, in whom he extirpated, he asserts, the entire parotid, preserving the facial nerve and the external carotid artery. In six weeks the cure was complete, and eleven years afterwards she remained in perfect health, presenting no indication of a return of the disease.—*Gaz. Méd. de Paris*, Jan. 5, 1861.

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35. *Removal of Inferior Maxilla for Osteo-Sarcoma.*—Dr. J. R. Dickson, Prof. Surg. in Univ. of Queen's Coll., Kingston, C. W., reports (*British Am. Journ.*, Feb. 1861), a case of this in a man 60 years of age, having a rapidly increasing malignant osteo-sarcomatous tumour extending along the entire left half of the inferior maxilla. The operation was performed on the 18th of June, 1855, the patient being partially under the influence of chloroform. The case went on favourably, and on the 22d of July the patient was discharged, apparently cured. On the 13th of the following Dec. the patient returned, with a reappearance of the disease in the cicatrix, and on the 16th of March, 1856, death terminated his sufferings, nearly nine months after the operation.

36. *Injuries requiring Amputation.*—Mr. SPENCE showed to the Edinburgh Medico-Chirurgical Society (Dec. 5, 1860) preparations from three cases in which amputation had been rendered necessary by injury.

The *first* was a very beautiful preparation of the bones of the lower extremity. The history of the case was this: Five weeks ago, a young man, walking on the top of an embankment of no great height, slipped his foot and fell, the leg being slightly bent at the time. He was at once brought to the hospital, when it was ascertained that he had sustained a compound fracture of the thigh, the periosteum being extensively denuded. In addition, there was a simple fracture of the bones at the lower part of the leg, involving the ankle-joint. The fracture of the thigh evidently extended into the knee-joint, and there was great laceration of the soft parts. Amputation was accordingly performed high up in the thigh. The preparation showed that the condyles of the femur had been broken up, that there was an oblique, almost longitudinal, and comminuted fracture of the lower part of the bones of the leg, that the ankle-joint had been opened into, and that a portion of the astragalus had been broken off. This case, Mr. Spence remarked, showed how a very severe injury might be occasioned by a comparatively slight fall.

The *second* preparation illustrated a rather rare form of injury. A child, six years old, while playing in the Grassmarket three weeks ago, was ridden over by a cart, the wheel passing over the arm. He was brought into the hospital in the afternoon, and was seen by the house-surgeon. The limb was swollen and very cold, and it was evident that the humerus was fractured. As the parents would not allow the child to remain in the hospital, the limb was put up very lightly, and directions were given, that if the swelling increased, the bandages should be loosened. On being brought back next day, pain had ceased, the limb was much swelled, and the hand was cold, dark-coloured, and mottled. Mr. Spence was unwilling to operate at once, but retained the child under observation. The limb was laid straight, and surrounded with cotton wadding; no bandage was applied. Two hours afterwards the hand was still cold; the mottled appearance was not increased. In the evening, it was evident that there was no attempt at recovery; the limb was cold and dead. Amputation, accordingly, was performed through the surgical neck of the humerus by means of an external flap. The preparation showed that the artery (which had been injected) had been ruptured opposite to the point of fracture, having apparently been pressed by the cart wheel against the sharp edge of the bone. Anastomosis not having been established, the limb had perished, not by gangrene, properly so called, but by direct death. Mr. Spence remarked that in many cases of fracture it was wonderful how the arteries escaped injury, being generally pressed out of the way of the bone. Here, however, all vascular supply had been fairly cut off.

In the case from which the *third* preparation had been obtained, a severe burn of the arm, forearm, and trunk had been sustained three months ago. After the general health had been somewhat re-established, it became evident that such destruction of the true skin of the arm and forearm had occurred, that there was no chance of a satisfactory recovery, for even if cicatrization took place, such contraction as was unavoidable must render the limb useless. Latterly the health of the patient was evidently giving way, owing to constant discharge, and pain and irritation from the large weak ulcer. Amputation was accordingly performed at the shoulder-joint, the only flap obtainable, from the condition of the soft parts, being from in front of the articulation.

In the three cases described, the patients had gone on favourably.—*Ed. Med. Journ.*, Jan., 1861.

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37. *Tracheotomy in Croup.*—Dr. WATSON exhibited to the Medico-Chirurgical Society of Edinburgh (Dec. 5, 1860) the larynx and trachea of a child, æt. ten months, in whom, three weeks ago, he had performed tracheotomy on account of croup. In this case, when seen by Dr. Watson, suffocation was impending, the respiratory acts were attended by drawing in of the chest, and expiration was much prolonged; the surface was blanched and cold, the lips purplish, the face and neck œdematous. The opening of the trachea afforded complete relief

for twenty-four hours. At the end of that time, however, respiration again became obstructed, and on removing the tube (which was a double one, and had been repeatedly cleansed) a loose portion of false membrane was seen flapping up and down with each respiration. On removing this, and clearing the bronchi, by means of a pair of small dressing forceps, of more of the same exudation material, respiration became easy, and continued so for four hours, when symptoms of obstruction again recurred. Although relieved five or six times in this way, the frequent reproduction of the obstructive material, and the increasing weakness of the child, speedily proved fatal, the child dying thirty-eight hours after the operation. The preparation showed patches of croupous deposit both in the trachea and larynx. The bronchi were plugged with a quantity of thick glutinous muco-pus, and the lungs, except in the upper lobes, were completely non-crepitant and collapsed. In opening the chest an unusual phenomenon presented itself, viz., the emphysematous distension of the anterior mediastinum, by which the heart was partially displaced towards the left side. This appeared to have resulted from the air finding a more ready access to the expanding thorax through the wound, and external to the trachea, than through the obstructed bronchi.—*Ed. Med. Journ.*, Jan., 1861.

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38. *Suppression of Suppuration, and the absolute Disinfection of Wounds by the Permanent Application to their Surface of a Sponge soaked in a Chlorinated Solution.*—Dr. Hervieux extols the efficacy of sponges steeped in a chlorinated solution as an application to suppurating wounds. The following are his conclusions in regard to this method of treatment:—

1. The permanent application of a sponge steeped in a chlorinated solution to the surface of severe suppurating wounds has the effect of transforming them into healthy-looking sores of a vermilion tint, free from exuberant fungous granulations and from suppuration. 2. While this mode of treatment suppresses the suppurative process, it favours cicatrization, which is never more regular and more certain than in the absence of suppuration. 3. This application resolves in the negative the question whether suppuration is the method employed by nature to repair the physical lesion inflicted on the living parts. 4. Among all the disinfectants of suppurating wounds there is none more efficacious than the sponge soaked in a chlorinated solution, because it suppresses the very source of fetor—namely, suppuration and its products. 5. With very few exceptions, this application causes no appreciable irritation on the affected surfaces, or on the surrounding parts. 6. This application is most advantageously employed in the treatment of phagedenic gangrene, of the eschars which succeed to severe fevers, of eczematous, scrofulous, or bony ulcers, of hospital gangrene, of perineal laceration, and generally of all suppurating wounds of an unhealthy character.—*L'Union Méd.*, Oct. 25, 27, and 30, 1860.

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39. *Treatment of Blennorrhagia by Injections of Subnitrate of Bismuth.*—The subnitrate of bismuth has been lately recommended as a local application in blennorrhagia, but as the results were not altogether satisfactory, M. Mourlon has investigated the circumstances which prevent the success of this kind of medication. He found that the injections of the subnitrate of bismuth often irritated the urethra; and on testing the salt, he ascertained that it reddened litmus paper. In order to obviate the acidity of the injections, he caused the salt to be washed until it presented no acid reaction; and under these circumstances he has found the injection almost uniformly successful, and has introduced its use into the military hospitals. In a memoir published by M. Mourlon, he states that out of 37 cases, 32 were cured, and that the average duration of the treatment was twenty-one days. With the exception of four patients who came into the infirmary for relapses, all the cases presented blennorrhagia of five to eight days' duration, and they were all acute cases. In none of them did the inflammation extend to the deep parts of the urethra, so rapidly was it arrested in its progress by the subnitrate of bismuth.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Bull. Gén. de Thérap.*, Sept. 30, 1860.

OPHTHALMOLOGY.

40. *Operations for the Relief of Glaucoma, &c.*—There is no department of our science, in which more startling developments have been made within the last few years than in Ophthalmic Surgery. The impetus to this seems to have been given by Dr. Graefe, of Vienna, who announced about four years since that he had discovered a remedy for that, fortunately not very common, but hitherto intractable disease, glaucoma. This consisted in making an incision through the cornea as close as possible to the sclerotic, grasping through this opening with a forceps, the iris, drawing it out and cutting off a portion of it. The larger the portion of the iris thus excised, the better, it is said. How the general hyperæmia of the eyeball, existing in acute glaucoma, was to be cured by incising the eye, dragging out the iris and excising it, struck most reflective surgeons as an enigma. No satisfactory explanation was offered, all that was said was that the phenomena of glaucoma resulted from "intra-ocular pressure," and that the removal of this pressure restored the sight. This explanation could not bear examination, but opposition was tried to be silenced by the assertion that in numerous cases the operation had effected marvellous cures.

Some enthusiastic young English surgeons who saw Dr. Graefe operate, soon introduced the operation into England; it was resorted to at the London Royal Ophthalmic Hospital, and great success at first claimed for it. From the number of cases operated on, it would appear as if a convenient epidemic of the disease had sprung up. But a careful examination of the iridectomy operations reported by Dr. Bader, as performed within a little over two years, at the hospital just named, hardly justify the claims set up for it. We take the following analysis of Dr. Bader's report, from a paper by Mr. Hancock (*Lancet*, Oct. 13, 1860).

"Of 107 cases, 16 are admitted to have been injured by the operation. The eyeball in several cases had to be extirpated after the operation, in consequence of hemorrhage and the escape of vitreous humour. The suspensory ligament of the lens was ruptured in others, and the vitreous humour escaped; in one case, from difficulty of seizing the iris, the vitreous humour, with the hyaloid fossa and lens, escaped, followed by half an ounce of blood.

Of 72 cases, bleeding from the iris into the anterior chamber occurred in 12, in one case beginning a quarter of an hour, in another a day, after the operation, the latter being followed by suppuration of the globe; and one case required a counter-puncture, to allow the blood to escape.

In 3 cases, hard portions of transparent lens escaped at the time of operation. In another, the lens and capsule, with about one-third of the vitreous humour, escaped through the wound six hours after the operation. Some portions of the lens escaped in another case, and some vitreous humour also while removing the remainder of the lens with a scoop; whilst in 7 cases the lens became opaque, without any fragments escaping. The opaque lens, however, was subsequently extracted favourably in several of these cases.

Dr. Bader's tables include the result of operations upon 84 eyes. Of these—
2 were not so well after operation.
36 were not benefited.
Of the remaining 46:
At the time of operation—
7 had no perception of light. Of these—
5 recovered perception of light.
2 recovered the power of recognizing fingers, one being also able to distinguish distant objects.
22 had perception of light in degrees varying from faintest, faint, fair, to actual perception. Of these—
3 recovered improved perception of light in portions of the retina only.
5 recovered improved perception of light.
1 recovered the power of recognizing objects by a portion of the retina only.
9 recovered the power of recognizing objects.
4 recovered the power of reading: 2 small, 2 average type.

6 had perception of light by portions of the retina only. Of these—
1 was somewhat better.
2 obtained perception of light by whole retina.
2 recognized objects by portions of retina.
1 recovered the power of reading type of average size.
6 had perception of objects. Of these—
2, who had perception of large objects, were improved.
1, who could see small objects, such as keys, could read average type.
1, who could count fingers, acquired the faculty of reading small type.
2, who had no perception of distant objects, and could only count fingers with difficulty, recognized fingers and distant persons.
4 could read large type. Of these—
1 was improved.
1, accommodating at from 3 to 10 inches, reads average print.
2 recognize distant objects and read average type.
1 patient could see, but not read, large type:
Reads large letters and tells the time on a distant clock."

Mr. Hancock, imagining that the pathological and ophthalmoscopic appearances in glaucoma were due mainly to the constriction exercised by the ciliary muscle, reprobates the injury done to the eye by Graefe's operation, and claims to have attained better results by a simple division of the ciliary muscle. This he accomplishes, he says, as follows: "I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided." It is not easy to understand how that microscopic muscle—the ciliary—can produce any important stricture, or how a slight incision of a few of its fibres, if really effected, could relieve such stricture, while it seems more probable that, in the operation under consideration, a few fibres are in reality only separated. But Mr. Hancock claims for it much greater success than is attained by Graefe's operation, and adduces 16 cases which he considers demonstrative of this.

Mr. Nunneley, of Leeds, in a very recent paper (*Lancet*, Jan. 26, 1861), expresses his conviction that neither the theory nor the practice, as applicable to the theory of Mr. Hancock, can possibly be true.

"Supposing," he says, "the ciliary muscle were really hypertrophied and in a state of tonic spasm, how could it induce hardness of the whole globe of the eye, flattening of the cornea, and much intra-ocular pressure? Muscles unstriped as well as striped act only in proportion to their length, in the direction of their fibres, and towards their most fixed attachment. The fibres of the ciliary muscle, firmly attached to the scleroto-corneal junction, radiate outwards and backwards from this point upon the soft yielding choroid. The fibres are not circular, but straight, and not more than one-seventh of an inch long. They can have no action whatever upon the sclerotic coat, to which they are not attached posteriorly; they therefore cannot render it hard. Whatever effect they may have upon the choroid would rather be to pull it away from the sclerotic, and hence, if any effect upon the latter, to render it less tense. Though the ciliary muscle be called by some the tensor of the choroid, it can hardly act much in this manner upon this coat, whatever it may upon the ciliary processes. But even were this tensile action proved, and the muscle contracted to its entire length, the choroid coat is too extensive, too lax, and too yielding to allow of the muscle dragging the whole coat inwards with sufficient force to compress the retina and vitreous humour in any great degree, and it certainly could by no possibility cause any atrophy or cup-shaped depression of the optic-nerve papilla, as all agree does occur in glaucoma; since the cribriform part of the sclerotic coat is *outside* the choroid, and therefore altogether beyond even the theoretical power of the ciliary muscle.

"But even were this theory of compression by the ciliary muscle as true as it appears untenable, how could a simple puncture in the direction of its fibres interfere with the action of the entire circle of its fibres? A broad transverse incision in the direction of the corneal curve, by dividing the fibres, might act powerfully in proportion to the number of fibres divided; but that a simple mo-

mentary separation from each other of two or three adjoining parallel fibres, without any division of their structure (for such must be the effect of a fine, sharp, thin knife), could permanently arrest strong spasmodic action in an entire muscle spread over a large circle, it is impossible to conceive."

With regard to Graefe's operation, Mr. Nunneley says: "While no operation has ever been brought forward with more authority, nor more highly extolled by its introducer, whose deservedly great name has induced many surgeons in this and other countries to adopt it, and who have agreed with him in certifying to its complete efficacy in curing diseases hitherto almost incurable, so, on the other hand, perhaps none has ever been proposed, the *rationale* of which is so obscure, or the *modus operandi* of which is so little known and is so unsatisfactory. That cures, more or less complete, have followed the performance of it, we have the abundant testimony of several most competent and trustworthy men; indeed, like myself, most ophthalmic surgeons have seen improvement follow it. The reasoning, however, by which this result is attempted to be explained appears to me very much less satisfactory than the facts themselves. So much is this the case, that when it was first proclaimed that the removal of a large portion—the more the better[1]—of what had hitherto been supposed to be an important, nay, essential, tissue for satisfactory vision, and the injury of which in a much less degree would, in the great majority of cases, render a sound eye useless, would be found to be a perfect cure for an eye already almost hopelessly diseased, appeared so astounding, that, like many others, I waited before doing it until the reports of some of those who had more faith than I had gave the result as so uniformly successful that doubt gave way before recorded facts, and though unconvinced by the reasoning, longer resistance to them appeared like obstinacy."

All, however, "that iridectomy accomplishes, in the cure of acute glaucoma and glaucomatous diseases is," according to Mr. Nunneley, "in the greater degree and more permanent manner in which it affords relief to intra-ocular pressure than paracentesis," as previously performed, and he thinks that he has devised simpler means for obtaining this desired result.

"Observing," he says (*Lancet*. Jan. 19, 1861), "that the eyeball is often distended to the utmost limit which the comparatively unyielding sclerotic and cornea will allow, and that the pain and acutely distressing symptoms in the ball and about the orbit commonly occur in proportion to the rapidity with which the distension takes place, whether the disease be glaucoma, iritis, or choroido-iritis, and knowing that the most unyielding portion of the globe is the point of junction of the sclerotic, cornea, iris, and ciliary muscle, which may not unfrequently, in very decided cases of hydro-ophthalmia, be observed as a depressed ring between the bulging sclerotic and cornea, it occurred to me that division of this part would afford the desired relief, and that not improbably the good gained in Von Graefe's operation in reality depends upon the removal of the resistance of this part, and not upon the ablation of the iris. The result, I think, has been proved. I have waited before announcing it until I have had a sufficient number of cases as fairly to admit of a comparative estimate of it with iridectomy. I have now operated upon about fifteen eyes by the method of Von Graefe, and on certainly not less than twenty eyes in the manner I propose to describe. The result has been in favour of the latter operation. It has, so far as I can judge, afforded all the relief that the more serious proceeding has done, and appears to be free from its inconveniences. It is easier performed, produces much less deformity, inflicts much less mischief upon the eye, is followed by as much diminution of the intra-ocular tension, by as great or even greater increase in the corneal curvature, and the relief has been quite as permanent."

Mr. Nunneley describes his operation as follows:—

"The manner in which I have operated is to puncture the sclerotic coat with the point of a sharp, thin knife—a small cataract or very narrow, short bistoury answers very well—not less than one-eighth of an inch behind its junction with the cornea, and carry it on to about the same extent through the cornea, making

[1] "The excised piece must be as large as possible." Von Graefe, vol. v. p. 354, New Syd. Soc. Pub.

altogether an incision about one-third of an inch long. Care must be taken to pass the knife sufficiently deep to completely divide these textures, and yet not so deep as to touch the lens, which I once did, owing to the patient starting at the moment the incision was made. Care also must be taken not to make the incision too long. A larger incision in the sclerotic, besides unnecessarily wounding important tissues, is useless, and if carried too far towards the centre of the cornea, though allowing this afterwards to yield more, is bad, for it may allow the lens to be displaced into the aqueous chamber; and if the iris should adhere to the whole extent of the corneal section, as it is likely to do, particularly if a portion of its whole breadth has been removed, not only will there be dragging of it, but the section becomes opaque, and hence the field of vision is lessened. In making the section, if the point of the knife has been well kept in, the outer margin of the iris will be divided. Sometimes the iris bulges through the section. I have tried the effect of simply leaving the prolapsed iris in the wound, of cutting it off, and also of pulling out a larger portion, and cutting off a strip through the entire width. In this latter plan the operation more nearly assimilates with Von Graefe's iridectomy, only that the section through the unyielding tissues is made directly across their junction, instead of into or parallel with it, whereby a greater expansion in it is allowed, and not nearly so much of the iris is removed. If none of the iris be cut off or tied, the pupil usually recovers its circular form; if some be excised, it remains oval and attached to the corneal cicatrix in proportion to the size of the piece removed, but in a much less degree than would *à priori* be anticipated. The degree of deformity is very slight indeed.

"The spot where the incision is made, so far as the relief obtained is concerned, can make very little difference; but I have usually selected the centre of the lower corneal curvature as being likely to produce less noticeable alteration in the appearance of the eye; and, when both eyes are operated upon, as interfering less with accuracy of vision than any other. On the whole I think, in severe cases, the removal of some small section of the entire width of the iris advantageous, as affording a longer continuous drain of the aqueous humour than simple incision of the scleroto-corneal junction does. I have in one case only seen much bleeding into the aqueous chamber, and what blood is there effused is soon absorbed. In one case the iris gave out more blood than usual—perhaps twenty drops; but as this at once escaped by the section, it was of no moment, and was easily arrested by cold, wet cloths. In two of the cases in which I performed iridectomy, the quantity of blood effused into the eye was sufficient to fill the aqueous chamber; and in one of them it was a long time before being entirely absorbed. Usually, however, all trace of it is lost within a week, or, at most, ten days. In two or three of the cases severe pain in the side of the head ensued, which an opiate relieved; and in one instance, as I have before said, considerable inflammation; but in all other cases very little inconvenience followed."

There are other surgeons who have no pet operations of their own devising for the cure of glaucoma, who distrust all these operative procedures, and we must believe, until additional and more satisfactory experience is obtained, that it is safest to hold the mode of treatment now under consideration, if not of questionable utility, at least as requiring further evidence to sanction its adoption.

It will be seen from the succeeding article what very high authorities still entirely repudiate it.

The benefit claimed by Mr. Hancock to have resulted in glaucoma from division of the ciliary muscle, stimulated Mr. James Vose Solomon, Surgeon to the Birmingham and Midland Eye Institution, to enter upon "an experimental inquiry into the value of incision of the ciliary muscle in the treatment of certain diseases of the eye and disorders of its accommodation," and in a series of papers published in the *Medical Times and Gazette* (Nos. for January 19 and 26, February 9, and March 2), he gives us the results of his investigations. The operation which he has practised and which he terms "Intra-ocular Myotomy," is performed as follows: A Beers cataract knife is entered "at the corneo-sclerotic union and pushed through the pillars of the iris into the muscle the flat surfaces of the blade being opposed on one side to the sclerotic, and on

the other to the rim of the lens." The incision in the muscle is limited to two lines or two lines and a half. In some few cases he thinks the puncture may have been even less than one-sixth of an inch.

By this operation Mr. S. asserts that "the adjustment of the near-sighted eye for near and distant objects is increased." And he relates several cases of near-sightedness said to have been completely cured by this operation. But what is more surprising, Mr. S. claims to have cured "by such an operation" "asthenopia —a form of 'weak sight,' where the patient is unable to maintain the eyes in occupation upon near and small objects, except for small periods, and regains power by resting them with looking into distance." Now it must be remembered that according to the prevalent theory of asthenopia, which Mr. S. evidently adopts, the ciliary muscle has become incapable of a continued effort to draw the lens forward, in this affection, so as to adjust it for near sight, and after a short effort becomes relaxed, and allows the lens to fall back and thus the patient becomes far-sighted. The division of this muscle consequently weakens its power in near-sightedness and strengthens it in far-sightedness!

Furthermore, Mr. S. states that his observations led him "at an early period" "to a conviction that the *nutrition of the tissues of the eye was improved by the performance of intra-ocular myotomy.*"

Again he says: "In some cases of symmetrical myopia, the performance of intra-ocular myotomy upon the 'worst eye' has effected a corresponding improvement in the accommodation of the other, only that the reading distance was an inch shorter, and features were not seen by one or two yards so far as by the operated eye; congestion of the choroid has also been removed." He asks, "Does not incision of the ciliary muscle give promise of enabling surgeons to regulate, without a recourse to spectacles, the focal adjustment of eyes in which the accommodation is not the same?"

"The accommodation to objects between the nearest and furthest points of distinct vision is not in any degree interrupted, and for distinguishing human features, is made equal to that of a person whose adjustment is normal, namely, twenty-five or thirty yards."

He states, "moreover, conical cornea, which is a disease that is not permanently improved by paracentesis, derives considerable benefit from incision of the ciliary muscle. The sides of the cone become more flat, and the vision of the patient has been permanently improved in all cases (eight eyes) in which I have operated, and opacities when present have also been rapidly absorbed. These results appear to point to an improved nutrition in the cornea, and to a diminution of the secretion of aqueous humour, which it will be remembered is furnished by the iris and tips of the ciliary processes—parts which are immediately implicated in the operation.[1]

"Intra-ocular myotomy exerts a marked influence upon the circulation of the choroid, quite irrespective of any loss of aqueous or vitreous humour which may attend its performance; also in diseases where the globe is of normal tension, and devoid of those signs which are attributed, erroneously, as I think, to spasm or constriction of the ciliary muscle, such as conicity of the cornea, 'a circular depression at the point corresponding to the ciliary muscle,' and bulging of the sclerotica (staphyloma).

"If I can succeed by the relation of cases in establishing this proposition, it will follow that the operation must influence the condition of the vitreous and lens which depend upon the choroid for their nutrition, and which suffer in so marked a manner in glaucoma and choroiditis."

It would seem almost incredible that this little, delicate, semi-transparent muscle—the ciliary—only one-eighth of an inch broad, should be productive of so much and such varied mischief, had we not Mr. Solomon's experience to demonstrate the vast benefits resulting from a division of it. Nevertheless we must confess that we are inclined to wait until it shall be proved by further observations, whether it is the surgeon or the patient who derives most benefit from the operation.

[1] "The vision in the cases of conical cornea, which I have treated, has been better where the muscle was divided enocularly than by the other method; but more cases must be observed before this point can be considered as settled."

41. *Iridectomy and Glaucoma.*—Dr. W. R. Wilde, of Dublin, is, and with good reason we are persuaded, no believer in glaucoma and iridectomy, and it appears from the following communication that more than one English ophthalmic surgeon of note is of the same way of thinking. Dr. Wilde writes:—

"The senior member of the oculist art in England, the venerated and now venerable William Lawrence, whose writings on the subject are known all over the world, is opposed to the operation, and has thus expressed himself to the author of the review[1] in a letter dated 12th of August, which is now before me: 'This able and well-timed exposure of the iridectomy delusion is of great service to the public, not only on account of the mischievous character of the proceeding, but from the quarter of its introduction into England, which has given it undeserved respectability and patronage.' Mr. Dixon's opinion, in which he denounced the operation, is already before the public, and has been quoted in the review in question. In addition, I may insert the following letter just received from him:—

"'You ask me what is my present opinion of iridectomy? To answer this question I must go a little into details. If the operation is to be judged of simply by the general result of the cases in which it has been performed, I should say that no mode of surgical treatment, proposed within my remembrance, has been more frequently done without benefit, or *even with positive damage to sight.* The definition of glaucoma, especially of chronic glaucoma, is so vague, that those who have had but slight practical acquaintance with eye-diseases, and know glaucoma chiefly from written descriptions, are constantly liable to mistakes in diagnosis; while the comparative ease with which iridectomy may be performed encourages many to undertake it who would be deterred from attempting an extraction or an artificial pupil by the acknowledged difficulty of such delicate operations.

"'Cases have come under my observation in which iridectomy had been performed for so-called chronic glaucoma, but where the real disease was partial cataract, detached retina, or simple atrophy of the optic nerve. Nay, I have known instances of the reflexion from old corneal opacities, or even that from an opaque lens, dislocated into the vitreous chamber of a blind eye, to be mistaken for the peculiar tint of glaucoma.

"'It would be matter for deep regret if merely negative results had followed these needless operations; but it is truly lamentable to reflect that iridectomy, hastily undertaken without a previous careful diagnosis, has often transformed a partial into a complete opacity of the lens, in patients ill suited to undergo subsequent extraction, at the same time, perhaps, giving rise to serious internal hemorrhage.

"'Of course, it is not fair to hold the originator of a new invention responsible for all the excesses of his followers and imitators; and, therefore, it would be unfair to test Graefe's operation by the strange applications of it which rapidly followed the announcement of his discovery. One cannot but feel, however, how much mischief was done by the broad and unqualified manner in which iridectomy was first put forward as a "cure for glaucoma," without a due explanation being given of the limits of its applicability.

"'These limits appear to me to be narrowed to those cases of acute inflammation, characterized by the following symptoms: a sudden attack of pain, at first dull, then rapidly becoming acute, and assuming the character of neuralgia throughout the ophthalmic division of the fifth nerve; loss of vision within a few hours or days, the second eye being often attacked very soon after the first. Along with these subjective symptoms, the following, of an objective kind: great injection of the sclerotic, an irregularly dilated and fixed pupil, with peculiar slaty discoloration of the iris; an uneven and hazy condition of the corneal epithelium, and an unnatural hardness of the eyeball.

"'You well know how hopeless these cases have always been, coming on, as they so often do, in enfeebled and unhealthy persons, and extinguishing sight before our remedies have time to act. Now, in these cases, I have seen marked

[1] Glaucoma and Iridectomy Epidemic, in the 'Dublin Quarterly Journal of Medical Science,' August, 1860.

relief attend the evacuation of the aqueous humour through a large corneal opening, and so great has been the benefit of this proceeding that I have often found it unnecessary to interfere with the iris; but, in other cases, the acute symptoms have returned, and I have then removed a portion of the iris with permanent benefit. I have made my incision in the cornea quite close to the sclerotic, but not absolutely in the latter tissue, as recommended by Graefe himself. Neither have I removed such a large portion of iris as he does, for it seems to me that to establish a free communication between the anterior and posterior chambers of the aqueous humour is the aim of iridectomy, and that, if this is effected, all further removal of iris can only do harm by the after-deformity of too large a pupil.

"'But there seems to be a disposition just now to treat the iris with very little ceremony, for the latest German discovery consists in removing a cataract by making a *small* corneal section, and pulling out the nucleus of even a firm lens with a large spoon-shaped scoop, the iris being freely cut away as a preliminary step, by way of preventing prolapsus iridis, so troublesome a complication of the ordinary operation of extraction. Certainly a very effectual means of preventing prolapsus iridis; but what is to be said of the prolapsus *humoris vitrei*, which the spoon is so likely to cause when thrust behind the lens?

"'If iridectomy had been announced as an operation for relieving certain forms of acute inflammation of the eyeball, it would have conferred a real benefit on ophthalmic surgery; but brought forward, as it has been, as a "cure for glaucoma," extravagantly praised as forming a new era in surgery, and illustrated by such a multitude of incredible cases, it has proved a fruitful source of delusion and disappointment.'

"Mr. France, ophthalmic surgeon to Guy's Hospital, and already known by his writings as a sound practical oculist, writes to me as follows: 'I have been strongly tempted to write a note of acknowledgment to the author for the good service to the cause of truth, humanity, and rational surgery, which it is calculated to effect. I, for one, cordially approve the fearless exposure this paper contains of as transparent a system of loose unconsequent reasoning and disregard of results as has ever called down the reviewer's castigation. It is well that so spirited a protest should be made in the name of legitimate medicine, against this prevailing epidemic, which is, however, but one manifestation of a *cacoethes operandi* now reigning. Mr. Bowman has, it is stated by medical reporters, cured many cases of glaucoma by iridectomy; but he himself has never put forward to the profession a single specific instance thereof under, as stated by the reviewer, "his hand and seal." How many of these *cures* may be among the eighty-four eyes operated on in the fifty-five cases in Moorfields Hospital, the sad but not unexpected results of which have been so precisely set forth by Dr. Bader, is more than I can tell.' Mr. White Cooper has thus written to me: 'Apart from its being yet on its probation, I think that the operation of iridectomy is too lightly regarded; the impression appears to exist, among a large number at least, that it is a mere trick, and that no harm can possibly result from it; that serious consequences *must* and *do* result I know, and I certainly attach more importance to the proceedings than appears to be the case with many.' Mr. Haynes Walton, author of our best work on ophthalmic operative surgery, says, in a communication I have just received from him: 'The author of that article has done the world a favour, and I thank him.' Why, I would ask, should these gentlemen, together with Jüngken of Berlin, Minten of Stockholm, Sechel of Paris, Dr. Jacob, myself, and many others, protest against an operation which would *restore sight*, relieve suffering, advance science, and bring money into their pockets—if such a mode of cure were really true? The only answer I know of is that which the ignorant public bring daily against the 'prejudices of the profession' in the matter of Turkish baths, 'brandy and salt,' or such other ephemeral delusions.

"One word more before I have done. It was well known that Sir Benjamin Brodie's sight had been failing for some time past; the cause was believed to be cataract, a diagnosis to that effect having been made by a sound practical surgeon. When I was in London some weeks ago, it was rumoured that he had

been operated on for glaucoma! It would be unjustifiable in me to enter into the details of the case as they were rumoured about, but I may remark that the profound silence which was observed by the friends of the baronet, and others, as well as by the officers of the Royal Society, gave an air of mystery to the proceeding which was unaccountable, except upon the supposition that in due process of time this great triumph of iridectomy in glaucoma, submitted to by the acknowledged head of the profession in England, and the president of one of the first societies in Europe, would be trumpeted abroad, and forever crush the opponents of the new German operation. Pending the result, I refrained even from speaking upon the subject—although Lord Brougham had at the recent statistical congress, when apologizing to a large assemblage of medical men, and the public, for his not being present, 'regretted the *cause* of Sir Benjamin Brodie's absence.' Now, however, the matter is no longer in private keeping; it is public property. A *Taunton* paper having published an account of Sir B. Brodie's defective sight, &c., that statement went the round of the newspapers, when the following cautiously worded article appeared in the *Times*, and was copied into many papers: 'We are authorized to state that he has lately undergone an operation for the improvement of his sight, and that a satisfactory result is anticipated.' So say all of us; but in the last number of the *Medical Times and Gazette* we have a short leader on the subject of that announcement, to the following effect: 'Iridectomy was performed under chloroform. We deeply regret to say that the result is not so satisfactory as the leader in the *Times* would lead the profession to hope. The left eye may be believed to be much in the same state as before the operation, if anything slightly improved; but in the right or better eye, vision is quite lost. The ground of hope in this case is, that as there is *now* a cataract very evident in the right eye, that this is the cause of the impaired vision; that the eye is *not glaucomatous*, and that, hereafter, vision may be restored by extracting the cataract.' It is, therefore, manifest—if this be true—that in this case there must have been something very peculiar, either in the diagnosis or in the practice. The announcement, thus made public by a London periodical, will, I am sure, be received with extreme regret by the medical profession in Ireland, by whom Sir Benjamin Brodie's merits, in every walk of life, were duly appreciated. And should the case unfortunately turn out as conjectured, it will prove a severe blow and a heavy discouragement to the promoters of 'The Glaucoma and Iridectomy Epidemic.'"—*Dublin Hospital Gazette*, September 1, 1860.

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42. *New Methods of Curing Cataract.*—Mr. Tavignot, in a communication to the Academy of Sciences, proposes to cure cataract by puncturing the cornea at two opposite points of its circumference by needles, one of which is attached to the positive and the other to the negative pole of a galvanic battery. The two needles are approximated on the surface of the anterior capsule, and brought to a white heat by means of the battery. The capsule is thus destroyed, and by a little manipulation, says Mr. T., the whole of the opaque lens can be charred and broken up.

This *discreet* proposition is rivalled by that of Dr. M. Langenbeck, of Hanover, who has recently recommended what he calls *isolation* for the cure of cataract. This consists in the application of the concentrated rays of the sun to the lens, which is done by directing the focus of a burning-glass into the eye for several minutes, so that it falls exactly on the opaque lens. He repeats this operation three times within a quarter of an hour. He boasts of having cured nine cases by this plan.

We must not omit to add, that Prof. Weber tried this method on rabbits, in which he had previously artificially produced opacity of the lens, and, as might have been anticipated, in no one case with benefit, while several of the animals died from inflammation of the brain.

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43. *Artificial Cornea.*—It is stated (*Abeille Médicale*) that Dr. Heussor, of Richterschweil, has successfully inserted artificial corneas of glass in a young girl, blind from opacity of the cornea. The eyes, it is stated by the narrator,

bear this foreign substance very well and will probably do so hereafter. (*Mon. des Sc. Méd.*, 23 Feb. 1861.)

This operation was some years since proposed by Dr. Nussbaum, of Munich (see No. of this Journal for July, 1854, p. 95 *et seq.*), but it was not supposed that it would succeed in the human being, and we must still entertain doubts on the subject.

44. *Proptosis, with Goitre, Palpitation, &c.*—Dr. C. Handfield Jones read an account of a case of this before the Royal Med. and Chirurg. Soc. (Nov. 27, 1860). Dr. C. J. B. WILLIAMS said that he knew of no class of cases more curious, or more peculiar in their prominent symptoms, than that referred to in the paper. There was none in which the effects of treatment were more striking. At first, these cases seemed to present all the signs of excessive arterial action, requiring depletion, sedatives, or other lowering treatment; but we now know that they were only to be combated by powerful tonics and general support to the system. Their pathology had at one time puzzled him (Dr. Williams) much. Many cases had been presented to him under the garb of heart disease or consumption. The physiognomical character of the disease was too remarkable to be mistaken. The prominence of the eye was striking: the eyeballs projected beyond the brows, sometimes to such an extent that the eyelids did not close during sleep. The vessels on the forehead, face, head, and neck were more prominent, throbbing, and larger than natural. This feature led him to regard the pathological character of the disease as consisting mainly in a kind of aneurismal varix of the thyroid. Enlargement of vessels would account for the prominence of the eyes, which were also, probably, in some cases rendered more prominent by effusion into the orbit. The whole cerebral circulation was in the same state of enlargement, producing excitement and other symptoms of disturbance of the brain. This state depending on diminution of nervous power, and not on increased power, explained the use of tonics in the disease. The action of tonics was remarkable. It might be necessary in some cases in the first instance to allay pain and excitement by the use of sedatives; but it was surprising how well the patient bore tonics when so much excitement prevailed. He had found the best tonics to be those of a more astringent kind, such as the perchloride and the phosphate of iron, which acted almost as specifics in the disease. Formerly he had been in the habit of using the nitrate of silver, and other of the milder tonics; but little benefit ensued until the more decided course of action was persevered in. This, with the occasional use of narcotics at night, highly nutritious food, pure air, and a careful husbanding of the strength, soon effected a beneficial change. He related the case of a lady to whom he was called in the early stage of this affection (he might here remark that all the cases which he had seen occurred in females). This patient was highly nervous, and suffered from great excitement and pain of the head; the eyeballs seemed starting from their sockets. Temporary relief was obtained by the exertion of pressure on the eye, and binding a handkerchief tightly round the head. There was enlargement of the arteries of the forehead, face, and neck. Was this a case for tonics? The answer was in the affirmative. Sulphate of iron with an excess of sulphuric acid was ordered. Under this, with a local application of Fleming's tincture of aconite, the patient perfectly recovered in a few days. He inquired if any member had examined, post-mortem, any of these cases; and suggested that the arteries of the brain and the orbit should, when possible, be carefully scanned.

MIDWIFERY.

45. *The Value of Anæsthetic Aid in Midwifery.*—Dr. KIDD read before the Obstetrical Society of London, December 5, 1860, a paper on this subject. In obstetric practice, the instances where the author has found the inhalation of

ether or chloroform to be called for in an especial degree, and where anæsthetic aid has proved decidedly useful, have been cases of version, forceps, twins, convulsions, and crotchet operations. He has known chloroform used in puerperal mania, but its apparent effect is perhaps a coincidence, and not of a curative nature. The author referred the Society to his previous work on *Anæsthetics*, where the result of 360 cases of midwifery treated under ether, and 1,700 under chloroform, without accident from fatty heart, are described. Of these two agents (though there have been twenty-five deaths from ether in general surgery), he believes ether is superior to chloroform in relaxing rigid perineum in labour, and otherwise acting on the muscles of the uterus, in version cases particularly. There have been no accidents from chloroform in about 30,000 cases of midwifery conducted with the aid of these agents. The mode of applying chloroform in the lying-in chamber recommended was that which is adopted now by all the chief obstetric practitioners in Europe and America with whom the author has personally communicated on the subject. In midwifery practice, the error of using "mixtures" of ether and chloroform was explained, as a patient supposed to be inhaling a mixture is in reality inhaling pure ether, and there is a danger of confusion arising in mistaking one anæsthetic for the other. A new anæsthetic of chloroform and ergot mixed was also mentioned. Cases of twins, where the second child presents with an upper extremity, "the pains severe and continuous, so that it is next to impossible for the accoucheur to introduce his hand to turn the child," were first described, where chloroform is invaluable, if there be no contraindication of diseased heart, etc. The several indications in undilated os uteri for tartar emetic, liquor opii, or chloroform, from the result in actual practice, were explained. Next those cases of twins were pointed out where at one particular stage it is judicious to allow an interval of rest to the uterus; here ammonia and ergot act like a charm; not that chloroform is injurious, but it requires to be given before or after ergot, and not at the same time. The author said that if in journals half the attention were given in fact to these points that is given to quack compounds of chloroform, all would be right. As in some cases of twins, so is it in some cases of "tedious labour;" the patient requires an interval of rest to renew reflex action, and remove the effect of exhaustion, "false pains," emotions of a depressing kind, sleeplessness, etc. Thus, an inexperienced or constitutionally delicate young woman, with her first or second child, becomes alarmed, sleepless, etc., during her labour. Some indigestion, diarrhœa, or fright has hastened her labour before its time. The first stage of labour has been attended with suffering, followed by fatigue. Here the pains are ineffectual in advancing the labour; but if there be no diseased heart or other contraindication to anæsthetics, the author strongly advocates their administration in the manner pointed out in the memoir. How does sleep during labour differ from anæsthesia? Sleep, according to the author, occurs only where there is exhaustion of sensorial or muscular power; anæsthesia is best where there is no exhaustion; it is independent of sleep. The reflex power of Marshall Hall is the frontier or limiting line between sleep and anæsthesia; in sleep it is active, in anæsthesia absent. In tedious labour the agony and pain will not permit the poor woman to sleep. Even opium is followed but by a tedious intoxication, without sleep, for hours; but chloroform is not an intoxicant, and acts at once and quite as safely. A patient in ordinary anæsthesia may be said to be doubly asleep. This is what is wanted for a short time in this class of labours, as thus reflex, sensorial, and muscular power are renewed. Emotion also is removed out of the way by chloroform sleep; and by a confident, cheerful demeanor on the part of the accoucheur, he may effect as much in two or three hours by chloroform as he might in almost as many days by delay, and opium, and waiting for nature. Cases of versional delivery, with and without chloroform, were next minutely described and compared; cases especially of excessive sensibility of the os uteri and vagina, where the waters have long come away, and the uterus has closed with spasmodic force over the fœtal hand and arm. One well-known obstetrician has had 300 such cases; and he is every year more and more satisfied with the aid afforded by chloroform. The value of versional delivery and its *rationale* were also enlarged upon, as well as the much to be desiderated probability of the abolishing of craniotomy, and of many cases where

the forceps is unnecessarily and cruelly resorted to at present. Next to version cases, the usefulness of chloroform in forceps patients was explained, and directions given as to the mode of administration. Chloroform in abortion cases at the fifth or sixth month, as advocated by Dr. Tyler Smith, was also dwelt on; and even in cases of placenta prævia, as administered by Professor Simpson, Denham, and others. Chloroform, as facilitating delivery by means of version, has been tried with advantage to anticipate hemorrhage. Ammonia or brandy is first given, chloroform then administered, the feet brought down, and the remainder left to nature as the chloroform wears off. The treatment of particular forms of puerperal convulsion by chloroform was entered into and explained, especially its usefulness in that form common in poor unmarried women—convulsions the consequence of mental emotion, or epileptic excitement; but in the class of cases tending towards, or the result of apoplexy, and those with albuminuria, the use of chloroform or opium must be somewhat secondary, Dr. Kidd thinks, to the general treatment and general relief of the congested cerebral or spinal membranes. The author is not favourable to the use of anæsthetics in the simple hysteric varieties of the disease, hysteria being generally one of the contraindications to the administration of chloroform. The law of tolerance of chloroform in midwifery is not unlike that of the tolerance of ammonia, steel, bark, wine, opium, etc. All these medicines are of exceeding value where there is present shock to the nervous rather than to the vascular system. Chloroform is invaluable where there is exhaustion, debility, or shock, the result of great or long-continued pain; where there is loss of nerve force, or convulsions from excess of reflex irritability or pain, or mental emotion or excitement, etc. But chloroform is of less importance, as even wine, bark, iron, ammonia, etc., are of less use, where there is exhaustion, the result of hemorrhage, hectic, diarrhœa, exhausting suppuration, etc. Such diseases as chorea, asthma, hooping-cough, tetanus (not unlike puerperal convulsions), are blotted out by chloroform, no matter how apparently weak the patient may seem; but it is different with debility from hemorrhage or diarrhœa. The author's further experience of chloroform, in operation cases of craniotomy, vesico-vaginal fistula, ovariotomy, enucleation of uterine tumours, etc., in which he has administered it largely, was, in conclusion, recited.

In the discussion which followed the reading of this paper, Dr. GREAM said that, from some observations which had been made, it might be inferred that chloroform was but little used in midwifery in London; but he was sure that in no place was it more extensively employed, and he could positively state that amongst the upper classes it was almost universally employed, but with a general feeling of abhorrence of anything like intense insensibility. Surprise had been expressed by Dr. Barnes at the statement that so large a number of cases had occurred in which chloroform had been used in midwifery without accident; but there appeared to be no difficulty in understanding this, for it should be remembered that it was never right, nor was it required, to carry insensibility so far as in surgery; and he (Dr. Gream) was sure that all the fatality which had attended the exhibition of chloroform in midwifery had arisen from some little want of care, or from a want of appreciation of its power. When anæsthesia was first introduced, he had opposed, with others, the indiscriminate use of it, for he thought that at that time it was recklessly employed; and he believed that the present safe and efficient manner in which it was exhibited in London was the result, in a great measure, of the opposition offered to its former indiscreet and dangerous employment. It was a fact that no death from chloroform in midwifery had occurred in London; but allusion having been made to two authorities "beyond the Tweed," it was right to state that there the same immunity from calamity had not been enjoyed; yet he felt sure that no agent could be more safe, and none more beneficial, than chloroform in midwifery, when properly administered. He thought the author of the paper had unintentionally exaggerated the ill effects (if there were any), and also the good effects, of chloroform. For three reasons, he (Dr. Gream) thought chloroform most beneficial in labour: it removed pain, it rendered turning more easy, and it facilitated recovery; while the only detrimental effect was in protracted labour with pelvic contractions, where, by lengthening the intervals between the uterine pains, and slightly

diminishing uterine power, it might cause delay; but its advantages in all other respects made full recompense for this one drawback. In instrumental delivery of every kind it was most advantageous, and he had seen fewer cases of hemorrhage since he employed chloroform; indeed, he had exhibited it to patients who had habitually been subject to this occurrence, without hemorrhage supervening—a fact worthy the attention of theorists, but nevertheless a fact. Each person had his own way of administering chloroform. He had tried several plans, but of late years he had employed a common tumbler, in which he placed the chloroform, together with a clean pocket-handkerchief moistened with about two drachms of the fluid. This quantity ought to last two hours or more. The patient's face projected over the side of the pillow, and the nurse or the husband (strictly under his direction) placed the tumbler under her nose and mouth, at a distance of about an inch and a half or two inches, and thus the vapour rose perpendicularly towards her. She soon expressed a sense of giddiness; but the tumbler was still retained, until there was nearly an inability to answer any simple question put to her, and that should be the point beyond which no advance should be made. The chloroform should be now removed, and be replaced in a few minutes; and by thus being replaced and removed from time to time, while the pulse is felt and the respiration watched, a labour might be conducted through its stages without danger and without pain. He had never, during the number of years he had used chloroform, had one moment's anxiety as to its effect upon any patient to whom he had administered it. The object of the handkerchief in the tumbler was to prevent any chloroform from running out or escaping on the bed if the tumbler fell over. He had found that any handkerchief or machine with which it was necessary to touch the face had the effect of rousing the patient, and thus did harm.

Dr. Druitt said that he believed there were very few labours in which chloroform might not be used with benefit at some stage or other; and that even when all the earlier stages go on easily and well, it is the greatest comfort at the final moment when the head emerges from the outlet. He did not believe that chloroform predisposed to hemorrhage; on the contrary, he knew women who had flooded severely in early labours, when they had no chloroform, and who had been confined under chloroform subsequently without hemorrhage. Neither does it seem to retard uterine action after the first inhalation or two, provided that it be used in the small doses which reason dictates; nay, it removes that obstacle to uterine action which is created by excessive sensitiveness of the orifice and passages, and which causes uterine action to be abortive. He knew a case in which, after a very slight inhalation—enough to tranquillize, but not stupefy—the head was driven through, with rupture of the perineum, that external sensitiveness having been allayed which is a bar to uterine action. In cases of protracted labour from rigidity, such as happen to robust women who marry rather late in life, the blessing of chloroform was incalculable. These were the cases formerly treated by bleeding, tartar-emetic, and opium; and, in opposition to the author, both opium and emetic tartar, in minute doses, were admirable adjuvants in the proper cases. No amount of torture equalled that which many women endured from excessive uterine action and quasi-inflammatory rigidity of the os, and chloroform agreed well with any other proper remedy that might be devised. The only reservation he would make was, that chloroform should be used in the minutest quantity, and the minutest quantity sufficed. Two drachms were enough in most labours, a few drops at a time, to imitate the normal condition of labour; that is, a short snatch of refreshing sleep at the end of every contraction, and a little drowsiness beyond. He had met with two cases in which a very small quantity of chloroform produced symptoms of angina pectoris in women whose hearts were weak, and in such cases he thought that the risk ought not to be run.

Mr. Tyler Smith agreed with nearly all that had fallen from the previous speakers in favour of chloroform in obstetric practice. He thought it might be laid down as a principle in regard to its employment that, besides its value in allaying pain, it was useful in all cases, especially in operative midwifery, where it was desirable to moderate excessive action of the uterus, and to promote dilatation and relaxation. He held, on the other hand, that it was contraindi-

cated in cases where there was deficient action of the uterus—as in feeble and tardy labour from inertia, and in cases where hemorrhage was expected. He had seen it stop the course of labour midway, and he believed that post-partum hemorrhage and retention of the placenta occurred more frequently after its use than without it. One good effect of the discussion on the present paper lay in the difference of opinion which had been elicited. The causes of these differences would be studied, and the truth brought out. It could not possibly be correct that chloroform relaxed the uterus so as to facilitate turning, and made it contract so as to increase the difficulties of this operation; or that it could both cause and prevent hemorrhage. He had himself no doubt of its usefulness in difficult cases of turning. He had met with cases in which version had been accomplished by its aid, where without it the operation would have been utterly impossible. He had seen mania follow its employment, and he thought that in some cases the relation was that of cause and effect. He had also met with bad cases of rupture of the perineum under its use. The patients were relieved from pain, but volition was not suspended, and under these circumstances the violent and fearless straining efforts ploughed up the perineum by the fœtal head in the expulsive pains. It was of very great consequence to lessen, as far as possible, the dangers attending the use of this great and beneficent agent. The influence of fatty heart, alcoholism, and other conditions, in fatal cases, had been much debated; but there was one source of danger which, so far as he was aware, had not been dwelt upon. He referred to idiosyncrasy. He had known patients affected to a poisonous extent by ordinary doses of ether or chloroform. He knew two ladies, in apparently good health, in whom a few drops of chloroform would at any time produce repeated fainting. He suspected, therefore, that some of the inexplicable cases of death from chloroform depended on idiosyncrasy, and, before its administration, it would be useful if patients were tested as to their tolerance of its effects.—*Med. Times and Gaz.*, Dec. 22, 1860.

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46. *The Placenta and Membranes in Twin Pregnancies.*—Dr. SPAETH, of Vienna, has observed the condition of the fœtal membranes in 126 cases of twin pregnancy. The following was the result—

In 49 cases, each had a distinct placenta, chorion, and amnios.

In 46 cases, the placentæ were united; but each fœtus had a distinct chorion and amnios.

In 28 cases, the placentæ were united, and there was a single chorion; but each fœtus had a separate amnios.

In 2 cases, the placentæ were united, and there was a single chorion and amnios.

Where the placentæ were united, the line of junction was often indicated by a distinct depression on the concave surface of the organ and by scanty deposits of fibrin. The line of demarcation was present even where the chorion was single, and where vascular communications between the two placentæ were distinctly seen on their fœtal surface.

In none of the cases where the chorion was single was there the slightest indication of its having been formed by the fusion together of two originally distinct membranes. At the level of the line which separated one amnios from the other, the chorion was always perfectly smooth, without thickening or depression. In cases where the chorions were distinct but the placentæ united, the vessels were always independent and presented no anastomoses. On the other hand, vascular communications between the placentæ existed in two cases where the amnios was single, and in one where there was a doubt as to its being single or double: they were also present in seventeen of the twenty-eight cases where a single chorion enveloped two distinct amniotic membranes.

The anastomoses were always formed by tolerably large branches situated superficially on the inner face of the placenta. Hence, in the majority of cases where the placentæ are united, and where the chorion is single, there is free communication between the vascular systems of the two placentæ. The anastomoses occur sometimes from vein to vein, and sometimes from vein to artery. In one case, M. Spaeth noticed an anastomosis between an artery of one fœtus and a vein of another.—*Brit. Med. Journ.*, Sept. 8, from *Zeitschrift der Gesellschaft der Ærzte zu Wien.*

47. *Fatal Twisting of the Funis.*—Dr. Billi, obstetrical professor at the Milan Lying-in Hospital, gives in this paper the particulars of a remarkable case of twisting of the funis, in addition to two others which he has already communicated to former numbers of the same journal. By the first of these latter, he believes that he established the fact that knots of the funis may so closely constrict this organ as to cause the interruption of the circulation through it and the death of the child. The second paper related not to a case of knotting, but of twisting of the cord, likewise inducing the death of the fœtus. About the middle of the eighth month of pregnancy, the fœtal movements became excessively violent, and so continued for about eight days, when they suddenly ceased. Two days after this, the breasts became tumid, and there was febrile action. Labour-pains came on a month and a half after the fœtal movements had ceased, and the woman was delivered of a fœtus in an advanced state of maceration, the funis being twisted upon itself in an extraordinary manner. The portion of the funis within half an inch of its insertion was, in consequence of the twisting, reduced to six lines in circumference, and resembled a tendon in colour and consistence. Water, when forcibly injected, would not penetrate this narrow portion of the funis. The author, as well as other obstetricians, has met with cases of twisting of the funis upon itself without any injury to the fœtus; but he believes that the great amount of it here noticed, so great that it was not possible to completely untwist it, can only have resulted from rotary movements performed in one direction, which in this case would be favoured by the large size of the uterus. Endeavouring to explain the cause of this rotatory movement, the author states that the left lobe of the cerebellum was found to be a fifth smaller than the right, and suggests upon physiological grounds that this may furnish an explanation of the production of rotatory motions in one direction which gave rise to the twisting in question.

A third case which has come under the author's notice now enables him to pursue the subject. Ceasing to menstruate in June, the woman, a primipara, first felt the fœtal movements towards the end of October; and, by the middle of December, they had become so violent that she had to seek medical assistance on account of them. By the practitioner consulted, these movements were compared to those of a wheel violently turned within the abdomen. After continuing with unusual violence for a week, they suddenly ceased, and were not felt again, nor did the abdomen continue to increase in size; the breasts, however, became painful and distended for some days. A fœtus in an advanced state of maceration was born at the end of February. The placenta, which was slightly atrophied, weighed 7 oz. troy, and measured 16 inches in circumference. The funis, twisted upon itself, was 12 inches 6 lines in length. For 11 inches and 8 lines from its placental insertion, it was large, measuring 2 inches 3 lines in circumference, but for the remaining 10 lines, to its umbilical insertion, it did not exceed *two lines* in circumference, resembled tendon in consistence, and was impervious to injection of water. On examination of the cranium, the proportion of the cerebellum to the cerebrum was found to be a fifth less than it should be, the former being also more softened, and especially so on the right side.

Professor Billi then, reasoning from the deductions derived from experimental physiology in respect to unilateral lesions of the cerebellum, concludes that in these two cases the circular fœtal movements resulted from the irregular distribution of the nervous power of the cerebellum over the muscular sense. In the one case, this was due to a changed condition of one of its lobes; and in the other, to a preponderance of such change in one of these. The vast difference in size between the twisted portion and the remainder of the funis was due to the obstruction in the circulation in the former, and the consequent distended condition of the latter. The author reproduced the same condition experimentally, by imparting circular movements to a fœtus recently dead, while water was at the same time injected into the umbilical vein and descending aorta. The funis was 20 inches long and 12 lines in circumference. After the twisting, it became reduced, for an inch of its length from the fœtal insertion, to 9 lines in circumference, and was also then rendered impermeable. In the other 19 lines of its length, the circumference of the funis was increased to 20 lines.—*Med. Times and Gazette*, Jan. 16, 1861, from *Omodei's Annali*, vol. 173.

48. *Treatment of Nausea and Vomiting in Uterine Inflammation and in Diseases of Menstruation.*—Dr. E. J. TILT read a paper on this subject before the Obstetrical Society of London, Jan. 2, 1861.

Nausea and vomiting were said to be comparatively uncommon symptoms in uterine affections, but very distressing from the loss of strength and from the irritability and despondency which followed them. The fact of nausea and vomiting occurring so frequently in connection with otherwise healthy menstruation and with pregnancy was considered to explain why vomiting was a symptom of diseased menstruation; and their occurrence during amenorrhœa, dysmenorrhœa, and menorrhagia, in which the body of the womb, and more particularly its lining membrane, is implicated, was given to explain why nausea and vomiting are frequent symptoms of internal metritis, whether chronic or acute; whereas it was said to be extremely rare to meet with them when the neck of the womb was alone implicated, for they neither accompanied its various kinds of ulceration nor the catarrhal inflammation of its mucous membrane, which is the most common of uterine affections. Continued nausea was represented by Dr. Tilt as much more frequent than vomiting, most troublesome in the morning, going off after breakfast or dinner, increased by worry, excitement, the fatigue of dressing or talking, and being sometimes so irksome as to cause patients to refuse taking any food unless forced to do so. Some patients only vomited once or twice in the morning, others more frequently. One only vomited at menstrual periods, and then incessantly for two or three days, with but short intervals of rest. Another thought that she vomited all her food for a year; and in one case the vomiting was continued for eight years, killing the patient at last by inanition. In most of these distressing cases there were no symptoms of biliousness, the sickness being a reflex nervous phenomenon, as in pregnancy. Dr. Tilt stated that uterine treatment, such as leeches to the womb, or the application of potassa fusa cum calce, would sometimes suddenly check the vomiting for a period; that this result cannot be depended upon; and that besides the regular treatment of the uterine affection, it was necessary to adopt some other treatment to mitigate the patient's sufferings. Even when the patient presented little signs of biliousness, Dr Tilt advised, as a preliminary measure, a full dose of calomel, followed by alterative doses of blue-pill, to be continued for a week or ten days. This would sometimes very much diminish the vomiting and nausea; if not, the well-known minor remedies for sickness might be tried in succession. Strychnine was also mentioned as having been useful with some patients; and various interesting cases were related, showing the utility of a solution of morphine, given in effervescing draughts, and repeated after every fit of vomiting, two grains having been, however, sometimes given without quelling the sickness. Blisters to the pit of the stomach, dressed in the usual way, or with acetate of morphine, were favourably mentioned; and, as a last resource, Dr. Tilt advised an issue to the pit of the stomach, by which means he was able to check vomiting which had lasted incessantly for a year in a patient who, last winter, was only kept alive by brandy. The issue had been discharging for six months, and still continued to check the sickness, notwithstanding a severe relapse of internal metritis, which had caused this distressing symptom. In another case of chronic inflammation of the womb, vomiting seemed to relieve the still more distressing pains, so Dr. Tilt did not think himself justified in recommending the application of an issue. When nausea was protracted, he urged the necessity of forcing patients to take a few mouthfuls of food repeatedly in the course of the day, as in the sickness of pregnancy; and he advised those who suffered from morning sickness to take a little tea, milk, and rum or brandy on waking or before getting up.—*Med. T. & Gaz.*, Jan. 19, 1861.

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49. *Puerperal Vaginitis.*—M. BEAU calls attention to a severe form of vaginitis which is sometimes met with supervening on the puerperal condition, which may readily be mistaken for gonorrhœal discharge. It is not very rare, and the women have some difficulty in getting their account of its origin believed. M. Beau, by careful observation, has satisfied himself of the purely puerperal character of the discharge. Like gonorrhœal discharge, it resists treatment

obstinately. He finds a solution of nitrate of silver to be the best application. *Gaz. des Hôpitaux*, No. 1, 1861.

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50. *Disease of the Umbilical Vein.*—Dr. Alex. R. Simpson showed to the Obstetrical Society of Edinburgh (Dec. 21, 1860) a preparation of an umbilical cord, in which the vein was seen to have undergone a peculiar kind of degeneration, such as he had nowhere been able to find described as occurring in this particular vessel, and which gave it the appearance of being affected with atheromatous degeneration of its inner coat. But, on microscopic examination, it was found that the morbid change that had occurred was seated chiefly in the muscular coat, and consisted of a kind of fatty degeneration of the muscular fibres. The child to which the cord belonged had arrived to the end of the fifth month, and was born dead; and its death had perhaps been due to the diseased condition of the umbilical vein, as the placenta had been healthy, and no morbid change could be detected in the body of the fœtus.—*Ed. Med. Journ.*, Dec., 1860.

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51. *Pyæmia in an Infant.*—Dr. William Zeigler related to the Obstetrical Society of Edinburgh the history of a case where pyæmia had sprung up in an infant a few days after birth. The disease seemed to have taken origin in ulceration and inflammation occurring at the umbilicus, immediately after the decadence of the cord.—*Ed. Med. Journ.*, Dec., 1860.

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52. *Death after the Injection of an Ovarian Cyst with Iodine.* By Dr. Reinhold Löwenhardt, of Prenzlau.—Mrs. R., forty-one years of age, the mother of several children, menstruating regularly, a midwife, and pursuing her laborious occupation with scarcely any intermission, came under my care two years ago on account of a very considerable enlargement of the abdomen, which did not cause her much inconvenience, and which was found on examination to depend upon the presence of an ovarian tumour. The tumour appeared to consist of a single cyst, to be non-adherent, and to be situated on the left side, although the patient persisted in ascribing its origin to the right side, to which she had repeatedly applied leeches on account of attacks of pain. I punctured the tumour four times, at gradually diminishing intervals, with the effect of giving exit to a clear watery fluid, containing a large proportion of albumen; the more accurate exploration of the abdomen, which was rendered possible on these occasions, confirmed the previous diagnosis. The general condition of the patient during this time remained pretty satisfactory, although unmistakable symptoms of anæmia had presented themselves.

Keeping in view what must be the ultimate result, and believing that this case was in every respect well adapted for being operated upon with a view to a radical cure, I proposed to the patient the employment of iodine injection; considering, from the accounts I had read of it, that it was the least dangerous of the means at my disposal to obtain the desired end.

The patient gave her consent, and, after emptying the bladder and rectum, I chose, as the point of operation, the linea alba, halfway between the navel and the pubis—the same point, in fact, where I had punctured on the former occasions. Any interference per vaginam was rendered impossible by a slight arching forward of the tumour in this situation.

About a fortnight before the menstrual period, I made a puncture with a straight trocar, the canula of which was two inches long, and scarcely two lines in diameter, and into which I introduced a horn tube one line in diameter, which projected an inch and a half beyond the canula. I drew off about ten quarts of fluid, when the belly fell into loose folds, and no more could be got out by pressure. Into the projecting end of the horn tube, I then introduced a syringe, and very slowly injected a fluid consisting of two ounces of tincture of iodine, two ounces of distilled water, and ten grains of iodide of potassium. I only allowed the fluid to remain in the abdomen (which I rubbed gently, to insure the proper application of the injection) for four or five minutes, as, from the entrance of the first drop, there were manifestations of severe pain, coupled with repeated asseverations of a sensation of approaching death. This appeared to me the more extraordinary, as I knew that the patient was not an unusually sensitive person;

nevertheless, consoling myself with what often happens in the case of the injection of hydroceles by the most skilful surgeons, and with certain reminiscences of West's excellent book on Diseases of Women, I allowed the solution of iodine to flow out as completely as possible, and removed the canula.

Even although the countenance remained pale, the sense of impending death continued, the pain did not abate, and the pulse could with difficulty be felt, I ascribed the whole train of symptoms to the temporary depressing influence of shock, gave analeptics, ordered the application of a wet bandage, and hoped for the best.

At the end of the first hour after the operation, however, the alarming symptoms remained in full force; there was repeated vomitings, the extremities were cold and covered with a clammy sweat, and the pulse was almost imperceptible. After effervescing draughts with morphia, slight inhalation of chloroform, and wine had been employed with scarcely any effect; after the patient had complained alternately of violent desire to micturate, of pain in the loins, in the right shoulder, and especially in the abdomen, as well as of a sensation as if the menses were coming on, death occurred at four o'clock in the morning, fourteen hours after the operation. Consciousness was perfect to the last, and it is worthy of remark that the patient, very soon after the operation, had predicted with great decision that she would die at this hour.

The publication of the unfortunate result of the operation seemed to me called for, on the one hand, because the method practised appears to be coming more and more into vogue, and consequently makes accurate statistics desirable; and, on the other, because in the books I have had an opportunity of consulting, I have not found a case in which a simple injection has been followed in so short a time by death, and in which no important anatomical change has been met with to account for the fatal result. In conjunction with Dr. Pahl, I examined the body thirty-six hours after death. Connected with the left ovary was a non-adherent cyst, almost entirely collapsed, attached by a pedicle about the thickness of the finger; the walls were not very thick, there was no trace of inflammation in its interior, and it contained about a pint of a dark-brown fluid containing iodine. The wound made by the trocar was entirely closed; a little bloody effusion surrounded the spot. The right ovary was occupied by three small cysts; the lining membrane of the healthy uterus was decidedly congested, and covered with bloody mucus. In the cavity of the peritoneum was a small quantity of a light-brown fluid, which contained a small quantity of iodine; there was no trace of inflammation of the peritoneum, of the stomach, or of any of the other abdominal organs.

It accordingly appears to me, although I have come to this conclusion with reluctance, that the probable cause of death was the violent shock to the nervous system caused by the injection. That this is often very considerable, is generally recognized, and that it was the cause of death in this case seems to be proved both by the fact that the alarming symptoms which made their appearance at the very beginning of the injection continued till death, and by the absence of any recognizable alteration in the organs involved; that in cases already published, similar symptoms have disappeared after at most a few hours, cannot be used as a counter-argument. Now, as the resorption of the iodine cannot be charged with the unfortunate result, but simply the irritation, at first local and then general, caused by the solution, I intend in future only to employ irritating injections while the patient is under the influence of chloroform, and to keep up this influence, in a moderate degree, for some time after the operation.—*Edinb. Med. Journ.*, Jan. 1861, from *Monatsschrift für Geburtskunde*, Oct. 1860.

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53. *Rupture of the Uterus.*—LEHMANN states that of 7000 women delivered in the "Gebäranstalt" at Amsterdam, rupture of the uterus occurred only three times: in private practice he has observed eight cases. In all the rupture took place during labour. It is in the third or fourth stages of labour that the uterine wall gives way, seldom until after the membranes have been broken. The author divides cases of rupture of the uterus into two classes—the spontaneous, and the accidental or mechanical ruptures. Under the first head are included

cases where the cause is a pathological condition of the uterus itself, a degenerated state of the uterine tissue, fibroid formation combined with irregular contraction, or abnormal thinness of the uterus, softening, hysteromalachia, or thickening of certain parts of the fundus and body combined with great tenuity of the cervix. But rupture may occur when these pathological conditions are absent, as in cases of pelvic deformity, disproportions between the size of the child and the pelvis, transverse presentations, or in cases of hydrocephalus in which the pains are extremely violent. Here, however, the uterine tissue is rarely in a normal condition at the time of the rupture. The author believes that rupture only occurs in such cases when part of the uterine wall has, in consequence of pressure, undergone contusion, softening, detritus, or even gangrene. To the accidental or mechanical class of cases belong those in which the rupture occurs in consequence of operations badly or roughly performed, such as turning, perforation, the use of hooks, tearing of the vagina by splinters of bone, &c. Rupture of the uterus occurs more frequently in multiparæ. The lower segment of the organ is generally the seat of the rupture: the placental surface is not more liable to rupture than other parts; it occurs more frequently on the posterior than the anterior wall; its direction is more generally horizontal than longitudinal; the body may be entirely torn from the neck. The edges of the rupture are jagged, the extent various. There may be simply small perforations in a portion of the uterus, the texture being there softened from continued pressure. The rupture may be complete or incomplete; in the latter case the whole thickness of the uterus is not torn through. The cervix uteri and the vagina are frequently involved in the rupture; more rarely the bladder and rectum. The rupture occurs suddenly, severe pain in the abdomen, followed by cessation of pains, escape of blood from the vagina, fainting, pulselessness, coldness of extremities, &c. The abdomen is changed, the parts of the fœtus can be felt. Emphysema of the cellular tissue the author has not observed. The *prognosis* is very unfavourable, death generally occurring soon after the rupture. In rare cases the fœtus has been retained in the abdomen, as in a case of extra-uterine pregnancy. In reference to the prophylaxis, it is recommended that the uterus be supported from without. As to the treatment, the fœtus is to be extracted as quickly as possible; if it have escaped into the abdomen and is alive, the Cæsarean section is to be performed.—*The New Sydenham Soc. Year Book* for 1859, from *Mon. Sch. f. Geburtsk.* xiii.

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54. *Induction of Premature Labour.*—Germann records, as the basis of an elaborate paper, twenty-three cases of the artificial induction of premature labour, out of 339 operations and 1019 labours, the large proportion of which he explains by the unusual frequency of rachitic pelvic distortion in the particular neighbourhood the scene of his observations, and his practice of ascertaining by examination the state of the pelvic diameters during pregnancy. The greater mortality for the children the earlier the operation is performed, is not dependent, he remarks, entirely upon the degree of pelvic contraction, state of development, &c., but is partly explainable by the fact that the presentation is often found defective in operations performed too early. The general indications for the operation, to be gathered from the examination as to the state of the pelvis, are given. The *mode* adopted in the twenty-three cases was various; in only nine of them was one method exclusively adopted, viz., one by the method of Kiwisch, five by that of Cohen, two by that of Braun, and one by that of Scanzoni (mammary). In the other cases, a combination of more than one method was necessary. Cohen's method was employed singly or otherwise in nine cases, and of these nine cases the children were born alive in seven instances.—*Ibid.*, from *Mon. Sch. f. Geb.*, xii. and xiii.

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55. *Epileptiform Convulsions in the third week of the Puerperal State.*—Dr. Uvedale West reported to the Obstetrical Society of London (Feb. 6, 1861) a case of this. After some premonitory symptoms of headache, stomach and intestinal derangement, a lying-in woman was attacked, towards the end of the second week, with frequent fits of epileptiform convulsions, which persisted throughout the third week. There was slight albuminuria, and the fits themselves were

followed by a condition of hemiplegia more or less persistent, with other alarming symptoms. The patient recovered under the use of remedies directed, in the first instance, to the relief of the gastric and intestinal irritation, which was looked upon as the chief exciting cause of the symptoms observed, and, towards the decline of the disease, to the removal of the anæmic and debilitated condition in which the patient was left. The author concluded with the remark that this case, in which the patient recovered perfectly, and in which, therefore, there could not have been any cerebral mischief, notwithstanding the formidable appearance of the symptoms, shows how little we can rely on symptoms alone in forming a diagnosis in cerebral affections, especially when the case is compared with one, a report of which, by the author, was read before the Society last October, in which latter case, with less formidable symptoms, the patient died in a few hours, and serous effusion in the brain, with other cerebral disease, were found on a post-mortem examination.—*Med. Times and Gaz.*, February 16, 1861.

56. *Puerperal Fever complicated with Diphtheria, in which life was saved by the Tincture of Sesquichloride of Iron.*—Mr. ROBERT DRUIT reported the following example of this to the Obstetrical Society of London (Feb. 6, 1861). A healthy woman, aged 42, was attacked on the 7th July, after labour, with shivering and violent pain in the left leg. The pain soon left the leg, and seized the left side below the mamma. There were extreme restlessness, heat of skin, and prostration. On the third day of the illness, violent fetid diarrhœa was established. On the ninth day, the diarrhœa had continued incessantly; the motions thin, reddish, and offensive, passed involuntarily; the patient drowsy, wandering, and picking the bedclothes; when there came on great constriction in the throat, difficulty of swallowing, and copious salivation, together with a diphtheritic exudation at the back of the fauces. The most lavish administration of port wine, brandy, beef-tea, opiates, and quinine, had failed to stay the progress of the malady, and the patient was evidently sinking, when, at the suggestion of Dr. Frere, who was called in consultation, the tincture of sesquichloride of iron was given. The effect was decided and unmistakable. The contents of the alimentary canal were deodorized, and the diarrhœa ceased within twenty-four hours; and, spite of difficulty of swallowing and the throat complication, the patient rapidly became convalescent. The diphtheritic exudation extended over the posterior half of the soft palate. The upper lobe of the left lung was left consolidated; but in the twelvemonth which has since elapsed it has entirely recovered its normal condition. The quantity in which the tincture of steel was administered deserves notice. Three fluidounces of the tincture were swallowed in five days, besides an ounce wasted in lavements. During the first day that it was given, the patient took two fluidrachms every two hours. It is evident that these doses did good, and it is probable that smaller ones might have failed. —*Ibid.*

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

57. *On the Nature of Death from the Administration of Anæsthetics, especially Chloroform and Ether, as observed in Hospitals.*—Dr. CHARLES KIDD read an interesting paper on this subject before the Physiological Subsection of the British Association for the Advancement of Science. We are indebted for the following abstract of this paper to the *British Medical Journal* (Sept. 22, 1860):—

From data already collected and tabulated, it appeared not difficult to explain and to offer some new directions as to the nature of death from anæsthetics, a subject of serious importance in medical practice: and the author having collected and tabulated 109 deaths from chloroform and 24 from ether (including 2 from amylene), believes himself in a position to give an explanation of these accidents.

Of these 133 deaths, 90 occurred in male patients; and about half, or 43, were

females, though anæsthetics have been largely used in midwifery practice. Accidents in children, too, have been almost unknown, though chloroform has been used in such cases very extensively; this immunity from danger in diseases of children and women being very marked.

There have been already about from 250,000 to 300,000 operations of various kinds performed under the influence of anæsthetics, chiefly under chloroform, including every variety of serious surgical accident; several hundreds of the patients had been, for such very serious operations as resections of joints, large amputations, ovariotomy and other female cases, ligature of large arteries, etc., more than an hour fully anæsthetized; yet in this large mass of serious surgical operations, there had not been any well attested instance of death from stoppage of the functions of life, or narcotism of the system by the chloroform. On the contrary, fully 80 per cent. of the deaths, and nearly all the deaths from chloroform, had occurred in trivial operations, from very small doses, *suddenly* before the anæsthetic had produced its full effect. It was not sought to be argued (though not very clearly established) that death in the human subject might not be produced by long-continued inhalation of chloroform; it seemed only established that this was not at all the mode in which death had been observed to occur in hospital practice.

The author suspected that, anæsthesia once established in a favourable subject, respiratory action was diminished, and that inhalation or normal respiration continued on a diminished scale also, almost after the manner of hybernation in warm-blooded animals; but, respiration once disturbed, attended by spasm of the glottis through the recurrent laryngeal nerves, especially in strong healthy but nervous subjects, for trivial operations, as tooth-drawing, whitlow, strabismus, caustic to warts, etc., death might occur, and usually had occurred, from a few drops of chloroform. Nevertheless, a law of tolerance of chloroform had been long established in hospitals, that where a patient was labouring under old disease, pain of a neuralgic kind, etc., he bore chloroform best; patients with long continued stertor had taken thus a pint of chloroform in well regulated doses at a single inhalation; one lady had sixty-two ounces in twelve days for intense pain, with good effect; another, two hundred ounces in six months, also with beneficial result!

The best mode of examining the statistics for future use, according to Dr. Kidd, was twofold: first, inductively; and then by comparing certain large groups of facts loosely one with another, and forming other conclusions deductively for application in every-day practice.

Single positive instances, the author thought, did mischief logically in a subject so essentially deductive, as the examination of groups of facts connected with anæsthetics. A single instance in a purely physical science will determine the fixed atomic relations of a salt or oxide, or the diagonal of two forces, or measurements of angles, tangents, or equations, etc.; but in disease and discussions on disease, the single facts are, though positive, so varying and exceptional, the disturbing forces in disease so active and numerous, that we must have recourse to groups of similar facts, or facts tending in a particular direction (statistics), and see how we may reason from what is known to what is still dubious or unknown.

It seemed well established that excitation of certain parts of the excito-motor apparatus in the nervous system produced contraction or spasm of distant parts, such as bloodvessels, muscles, etc.; thus there are spasms of the parts of the neck, laryngismus, trachelismus, spasm of the glottis, and in actual practice under chloroform these are more to be feared than deep insensibility with stertor.

As to the somewhat casual or accidental nature of some of the deaths from chloroform, there was every year additional proof. Of 45 deaths collected by Dr. Snow, he believed five or six were from fright on the operating-table. Chloroform, it might be remembered, has now to bear the obloquy of all the preliminary mischief in operations formerly placed to the account of something else. It was curious that nearly all the deaths happened before operations, and seldom, indeed never, as the result of a long tedious operation. In 85 deaths classified, 17 were operations for hæmorrhoids, fistulæ, etc.; 14 were operations for removal

of toe-nail, necrosed finger-bone, etc.; 10 were cases of tooth-extraction; 11 were operations for removal of tumours and such like; 9 were deaths in minor amputations; 6 were strabismus operations, cysts of eyelids, etc.; 9 were operations on the testis; 5 were reduction of dislocations; 1 was hernia; and 5 were delirium tremens. The broad general fact that, in 300,000 operations of all kinds, chloroform had decidedly saved 6 to 10 per cent. of lives, also tended to show that the cause of death was of a casual nature.

Looking broadly at the facts, it was found by the author that the deaths from chloroform were all sudden, and many of the nature of "fit." Chloroform has a powerful irritant action on the pneumogastric in the lung; and a like irritation by electricity, as now known, stops the action of the heart. Hence syncope may possibly occur, if this irritation or spasm of the pneumogastric and laryngeal nerves be reflected to the heart.

Chloroform, as a general fact, had had a good effect on the general result of operations in surgery. Where it has acted badly, as shown by statistics, it has probably been from a tendency of patients themselves to put off surgical operations too long, changing primary into bad secondary operations. Probably 10 per cent. of lives are still directly saved by chloroform yearly (comparing the present surgical death-rates and those in existence immediately prior to the discovery of anæsthetics in 1846). This showed, according to the author, that the cause of deaths from chloroform must be also of a casual or accidental nature, rather than as arising *ex necessitate rei*. A large number (one-fourth) of the patients also in whom death had occurred had taken chloroform before with perfect safety; one patient, a hundred times. This also corroborated the view that the cause of death is more of a casual nature than due to idiosyncrasy or the general tendency of anæsthetics.

This fact of deaths from chloroform occurring in trivial operations, and early in the administration, or in the stage of excitement, had been remarked by all the chief observers—M. Robert of Paris, M. Denonvilliers, Mr. Paget, Dr. Snow, etc. It was believed to be due to diseased heart; but this opinion had not been borne out by any group of facts; while there was the larger negative clinical fact that hundreds of surgical patients with diseased heart have had chloroform with entire safety; and that, in by far the largest number of cases of death, the heart was healthy, and the symptoms were referable to the brain. Thus, in 18 deaths, the only instances where *post-mortem* or other examination has shown any lesion, 5 deaths were the result of delirium tremens; 8 had cysts in the brain, meningitis, etc.; 2 had chorea; and there were only 3 where traces of diseased heart were suspected or found. Then, of 45 deaths collected by Dr. Snow, he believed 6 were from fright or emotional depression on the operating-table, evinced by intense nervousness; while, as regards the 24 deaths from ether, they seem to have occurred from extreme muscular relaxation, exhaustion, and hemorrhage, favoured by this condition, after, not before, operations.

There had been probably twice as many patients saved from impending death by chloroform by proper restoratives, as the number that have succumbed to its influence. All these restoratives have been directed to excite the brain and reflex system of nerves; and it is to be feared some patients were lost by venesection, used on the theory of fatty or obstructed heart. The author had found that intoxication, or delirium tremens, and hysteria, strongly contraindicate the use of chloroform; and a fact of a very curious kind, belonging to the same category, had been somewhat recently dwelt on in the battles of the Crimea, in India, at Solferino, etc., that all nervous frightened prisoners are acted on with much difficulty, and not without danger, by chloroform; excess of emotion acting like excess of sensation, necessitating very large doses of anæsthetics—a state of "exaltation of sensibility" not very far removed from that which causes spasm of the glottis, trachelismus, etc., in fatal cases. As to delirium tremens, Dr. Snow does not seem to have suspected it; but of 45 deaths, the author collected (independent of suicide cases, all through these researches omitted), he finds 9 cases probably due to intoxication or delirium tremens, though some were put down to other affections or surgical operations.

In ten cases very carefully described by accurate observers, in four there was evident sign of the heart still beating, though the respiration had ceased—one

by Langenbeck; one by Mouatt in the Crimea, the single case of accident there ("death from removal of finger; patient became violent; spasm of larynx occurred; heart's action continued even after pulse and respiration"); and two others; so that it is not impossible that, if this phenomenon were watched, it would be detected more often. It appeared to the author, indeed, that we should never be safe in chloroform administration, if the heart, as he believes it to be, were not one of the *very last parts*, if not the last, to be depressed by chloroform; he rather feared implication of the respiratory tract, in the laryngeal nerves, *nœud vitale*, pneumogastrics, etc., and a disturbance of their delicate balance.

The chief conclusions arrived at were—

1. There is little, if any, benefit in choosing ether in preference to chloroform, as an anæsthetic. In "ether mixtures," the ether is first inhaled pure. Ether is to be especially avoided where we have excessive hemorrhage or muscular relaxation to fear; though in such cases as reduction of dislocations, and in midwifery, it has some points in its favour, but not in the shape of mixture with chloroform.

2. There is less cause of apprehension in administration of anæsthetics in the cases of children and females, and in serious surgical operations, provided the anæsthetic is administered skilfully, than in the cases of men of robust frame, especially if given to intoxicated habits, or the operation be connected with tendinous parts, so often followed by syncope without chloroform.

3. All hospital experience tends to corroborate the view that there is a very remarkable "law of tolerance" of chloroform observable in all bad surgical cases. Once the conjunctiva of the eyelids is insensible, the eyelids closed and normal, anæsthesia kept up with ordinary skill, there is a period of safety, with diminution of respiratory action, established almost like hybernation, but which leaves the heart unaffected. That this, rather than a single instance of death from diseased valves and chloroform; that this, seen in tens of thousands of cases, should encourage the most hopeful views on the diffusion of anæsthetics.

4. Idiosyncrasy has probably very little to do with these accidents, if we omit intoxicated habits, hysteria, tendency to "fits," etc.; so that "trials d'essai," so common hitherto, or placing a patient under several doses of chloroform on previous days to a surgical operation, is a mistake. A patient may have chloroform a hundred times, but die of its effects as surely on the next or hundred and first.

5. There are evidently two modes in which our common anæsthetics may cause death, which require watching; ether, at some distant but uncertain interval during the first twenty-four hours after a surgical operation; but chloroform, like lightning, in an instant, if the inhalation be not stopped. Probably in one half of the latter cases, at least, the heart, as in asphyxia, is still beating; the action being more like an irritant one on the laryngeal recurrent, and pneumogastric nerves; the other cases are instances of syncope, of convulsive fits, etc.

6. In several cases of death—as in delirium tremens cases, to wit—death occurs because ordinary restoratives do not act, as we have an imperfect reflex and nervous system to work with; but, in all cases of impending death, we are justified in at once having recourse to such means, viz., artificial respiration by pressure, as it acts also on the cavities of the heart; tracheotomy, if we have to fear this form of spasm or asphyxia (the engagement of the double respiratory centre of the pneumogastric being very puzzling under chloroform, but rather of the nature of spasm); sudden cold dashing of water, to wake up the respiratory nerves; fresh air fanned on the face, etc.; too much not to be done at first, as the spasm may subside. Acupuncture of the muscles of the neck is also recommended, so as to irritate the spinal accessory, and phrenic, but not the eighth pair of nerves.

7. Hitherto our experience of this spasmodic irritant condition, and our experience of oxygen gas, galvanism, etc., as restoratives, have not been encouraging; these agents being too irritating, and requiring very exquisite skill to apply them, especially galvanism, which hitherto has only, it is to be feared, done mischief. A better restorative seems to be the injection of warm wine into the rectum, or, as tried in animals poisoned by chloroform, transfusion of any simple saline fluid into the veins, as in the analogous collapse of cholera.

58. *On the Use of Oxygen Gas as an Antidote in Cases of Poisoning by Chloroform and Ether.*—M. OZANAM, after numerous experiments upon the lower animals, has arrived at the conclusion that oxygen gas is more efficacious than any other substance in neutralizing the effects of these highly poisonous vapours. The animals upon which he tried this antidote were aroused more promptly by oxygen than by atmospheric air, the difference in the time occupied being in every instance more than one half of the full period.

In other experiments, it was found that when the anæsthetic vapour and the oxygen gas were simultaneously inhaled, the length of time required for renpering the animal insensible, compared with that necessary for accomplishing a similar result when the atmospheric air was inhaled in the place of the oxygen, was as 3 to 1.

According to M. Ozanam these experiments afford a further proof that the various modifications of ether and chloroform act injuriously, in consequence of their being a source of carbon, which is readily assimilated, and which the oxygen disengages from the blood by facilitating its combustion.

This author has established so firm a reputation for the accuracy and value of his researches, that we believe that his suggestion, if properly carried out, will be found useful in preventing a repetition, in many instances, of those occasional disastrous consequences of the inhalation of chloroform which have, of late, made many practical men timid in employing it; oxygen gas can, as our readers are aware, be easily procured, provided the cheap and simple ingredients required for its production are at hand. It should be borne in mind that its efficacy is only apparent, as will be understood upon a slight consideration of the mode of death, in cases in which respiration has not entirely ceased, and not when collapse from a sudden, fatal, cessation of the heart's action has taken place.—*Lond. Med. Rev.*, Sept., 1860.

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59. *Rapid Delivery.*—Dr. JAS. C. L. CARSON relates (*Med. Times and Gaz.*, January 26, 1861) the two following cases, which show how necessary it is to be cautious in investigations regarding infanticide:—

"I was called out of bed in a great hurry one night to see Mrs. ——. Fortunately I had not far to go. On my arrival I learned that my patient had been roused out of a sound sleep with a feeling of urgent necessity to go to the close-stool. The moment she placed herself on the stool a violent labour-pain came on and the baby was born. Her friends were so much alarmed that they could render no assistance, and the baby would have been drowned in the water had it not been for the timely aid of a sensible lady who happened to be staying in the house that night. She got the patient gently raised to the edge of the stool and then supported the child on her hands till I arrived.

"Cases of this description are worthy of being recorded, as they are of great importance in a medico-legal point of view. I have a very distinct recollection of another instructive case which occurred under my charge when I was residing in one of the Dublin lying-in hospitals in the year 1834. The nurse called me to the bedside of the patient, as the labour was very strong, although she had not been more than half an hour ill. By one violent effort the child and placenta were expelled together, and the waters were not discharged. I tried to rupture the membranes with my fingers, but in vain. They were so unusually thick and tough that I had to open them with the scissors. I am quite confident if this woman had been alone at the time, her child would have been lost."

Another case is recorded by Dr. S. W. POOLE. He states (*Lancet*, Dec. 22) that very lately he was called to a woman who had given birth to a child. On arriving he found her in bed, and a full-grown child (her second) beside her. Twenty minutes before she was standing on the floor, aware that she was about her full time, but having had no warnings of labour. Suddenly she felt "a cramp in the stomach," and the child dropped at her feet. She had no pain after the birth of the child, but Dr. P. found the placenta close to the vulva, though so tightly grasped by the vagina that he had some difficulty in extracting it. There was no hemorrhage from the ruptured funis, and little or no blood about the person of the mother. The woman was young and healthy, and made a rapid recovery.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Gunshot Wound in the Mouth; Secondary Hemorrhage; Ligature of Common Carotid Artery; Recovery. By CHAS. R. S. CURTIS, M. D., of Chicago, Ill.

On the 19th of April, 1857, about 8 o'clock in the evening, I was summoned in haste to a consultation with my friend, Dr. J. H. Ledlie. The history of the case was as follows: The patient, an Irishman, some ten days or two weeks previously, in a drunken row, had been shot with a pistol, the ball entering the mouth, knocked out the lower incisors, and, penetrating the tongue, passed backwards and outwards to the angle of the lower maxillary bone. Striking the bone, it was turned from its course, and, passing directly backwards, buried itself in the deep fibrous tissues of the neck. Dr. B. McVicker, of this city, was sent for, and attended the case. He was, however, unable to remove the ball, and it was left to come away by the suppurative process.

The patient, I believe, appeared to be doing very well up to the afternoon of the 19th, when a sudden and violent hemorrhage took place from the orifice of the wound. The attending physician was immediately sent for, but, as he could not be found, Dr. Ledlie was called in his stead. The doctor, after having adopted a variety of measures to stop the flow, and finding them unavailing, sent for me.

By the time I saw the patient, he had suffered much from loss of blood, and was still bleeding profusely. It was evident from the colour of the blood, and from the fact that the intensity of its flow corresponded with the pulsations of the heart, that the ulcerative process around the ball had penetrated an arterial branch of some magnitude, and, consequently, we could expect little from ordinary styptic remedies. I, however, suggested to the doctor that we should make one more effort to arrest the bleeding, before resorting to operative interference. Accordingly, we proceeded to plug the wound with fine lint dusted with tannin, after the manner of the tampon. The lint was pressed in tight and close as far as to the angle of the inferior maxilla, and apparently at first with perfect success. The hemorrhage was stopped. The patient was cleaned up, and I was about to retire, leaving the patient in the hands of Dr. Ledlie for the night, when he suddenly notified us that the bleeding had recommenced. No further time was to be lost. Our patient was already much reduced. The doctor concurred with me in the opinion that the only alternative left us was to ligate the common carotid artery as soon as possible.

Accordingly, the patient was laid upon his back, with his head depressed and a little inclined to the left, so as to place the right side of the neck in as fair a position for operating as possible (the ball having entered on the right side). I then proceeded to make an incision through the skin and superficial cellular tissue, from a point over the anterior edge of the sterno-cleido-mastoideus and on a level with the pomum Adami, downwards about

two inches towards the sterno-clavicular articulation. The next incision penetrated the platysma hyoides, and exposed the omo-hyoid, the sterno-cleido-mastoid, and a part of the sheath of the vessels. The omo-hyoid was readily pressed to one side, but, owing to an unusual development, some of the fibres of the sternal portion of the sterno-cleido-mastoideus were divided and pressed to one side, so as to fully expose the sheath of the vessels. The decendens noni nerve was observed and avoided. The sheath was carefully raised and opened, and the artery, nerve, and vein were found *in situ*. An aneurism needle was now introduced, armed with a strong ligature, between the artery and vein, and cautiously passed around the vessel, carefully avoiding the pneumogastric nerve. The instrument being now withdrawn, a double knot was applied and the ligature drawn until the inner coat of the artery was distinctly felt to yield. The ends of the ligature were suffered to hang out, the edges of the wound were brought together by the interrupted suture and adhesive plasters, and the tepid water dressing and an anodyne ordered. The operation was followed by no remarkable disturbance of the arterial or cerebral functions. The hemorrhage ceased almost instantaneously, and the patient was left for the night in a very comfortable and quiet condition.

20*th*. The patient doing nicely. No further hemorrhage; pulse one hundred. Continued the tepid water application. Ordered him to maintain the recumbent posture, to observe a low diet, and to take the following: ℞.—Liq. potass. citrat. ℥ij; aquæ camph. ℥iij; spts. eth. nitrici ℥j; morph. acet. gr. vj; antimon. tart. gr. iv.—M. One teaspoonful to be given every hour.

I now left the patient in the hands of Dr. Ledlie, who continued him on the above treatment, occasionally ordering a mild cathartic. The doctor frequently reported him to me as doing well up to about the end of the second week, when we visited him together and removed the ligature, the wound in the neck having healed kindly by the first intention, except where the ligature passed out. At this time, there was a good deal of inflammation and swelling about the jaw and neck near where the bullet was supposed to be lodged. Suppuration was undoubtedly progressing, and, in order to favour its progress, outward warm fomentations were ordered. These were continued for several days, when we again visited him, and the abscess, having begun to point, was opened with a sharp-pointed bistoury. A good deal of unhealthy pus escaped, but the bullet did not come away for several days. But when it had passed off, the abscess healed kindly, and in a few weeks the patient was able to go about his business as healthy and strong as ever.

Some of the most interesting circumstances in connection with this operation were that it was done in a case of emergency, with but one professional assistant, without the aid of chloroform, and with no other light than what a miserable article of tallow candles could afford.

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Strangulated Inguinal Hernia successfully treated by an Enema of Tobacco Decoction. By BEVERLY P. REESE, M. D., of Ford's Depot, Dinwiddie Co., Va.

On the night of the 29th September, 1858, I was summoned in great haste to visit a Mr. V., who, the messenger said, had been taken suddenly ill while at the supper-table and had fallen from his seat, and his wife had sent for me, with urgent solicitations to get to his aid as soon as possible. Upon my arrival, I found the patient lying on the floor, behind the door,

with his pantaloons nearly off, revealing at once the malady. He appeared in such agony that I immediately resorted to the lancet, thinking, from the distended appearance of scrotum, it would be impossible to relieve him before overcoming the muscular contractility of the parts. I bled him nearly to syncope, and then resorted to taxis without success.

Finding him difficult to manage without aid, I immediately ordered a physician who lived near by to be sent for, using during the interval warm baths, nauseants, &c., and taking advantage of position, so far as I was able, to control him by the assistance of his wife. I think I never saw an individual suffer more for the time, it being difficult to keep him from going into the fire, saying he preferred to burn than suffer as he did. In a short while assistance was at hand, and all the manipulations we could use proved to be of no effect. Finally, I inquired if he had a syringe; he answered it was at a near neighbour's. I immediately sent for it, with orders to bring some tobacco with it. With great dispatch it was brought, and I immediately made a tobacco decoction, and gave him an enema of about half an ordinary sized cupful, and placed him in a warm bath. In a few minutes, his lips began to grow pale, with a general relaxation of the muscles of the face. He, thinking that he was dying, requested his children to be brought around him, in order that he might give them a parting farewell. I asked the assistance of the doctor, who took him by the feet and I under the arms and placed him on the floor, where, with little or no difficulty, the hernia was reduced.

I have given this hurried sketch of the case: 1st. As an evidence that relief may often be afforded by remedies at hand, without resorting to the knife and severer and more hazardous treatment. 2d. To show the beneficial effects of the use of tobacco, if we could only control or rather regulate its influence.

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Chloroform and Tinct. Opii in Tetanus; Recovery. By Jas. L. Ord, M. D., of Santa Barbara, California.

In your number for October, 1860, there is reported a case of spasms which was cured by the use of chloroform, both externally and internally. The author leaves it to the future to determine whether the internal administration of chloroform is as important in the treatment of spasmodic contractions as analogy had led him to suppose.

The following case came under my care two years since, and was noted in my case-book, which goes to show that chloroform may be used with safety and success in spasmodic diseases of the worst type.

A. F. H., aged about 36, of a robust constitution, sanguine temperament, by occupation a merchant, had, by accident, the prong of a pitchfork thrust through the palm of his left hand about 5 P. M. He called at my office at 8 o'clock the same evening, suffering intense pain in the hand, extending up the arm to the shoulder; at the same time felt a kind of numbness, so much so that he conld only move the arm by taking hold of it with the right hand. The jaws were quite stiff, and he could open his mouth only enough to drink. His articulation was also impeded, and he was labouring under considerable nervous excitement. From all the symptoms, he had traumatic tetanus, and there was no time to lose in treating it. I made a mixture of equal parts of tinct. opii and chloroform (℥iv āā), and gave him a teaspoonful in half a tumbler of water, and told him to take the same quantity every hour until relieved; then applied a flannel cloth, moistened with the same mixture, to his arm and hand, covered it with

oiled silk to prevent the rapid evaporation, with directions to keep it wet until the pain left. Next morning early I visited him, and was glad to find that his jaws had relaxed to their normal condition, and all the pain in the hand and arm had left. He had taken four teaspoonfuls of the chloroform mixture before he obtained any relief, and did not get to sleep until about 2 o'clock A. M. next morning; had applied the mixture to his arm three times before the pain abated. He appeared almost well, and was but little under the influence of the medicine he had taken. For several days, there was a numbness of the hand and arm, but, by using the mixture at intervals externally, it gradually left him. He entirely recovered before the end of a week from the time he was injured.

Cherry Laurel Water in After-pains. By SAMUEL R. RITTENHOUSE, M. D., of Macungie, Lehigh Co., Pa.

I desire to call the attention of the medical profession to the use of cherry laurel water in those distressing pains to which many women are subject after parturition. In the great majority of cases, so far as my experience goes, this medicine, if given in doses varying from half to a fluidrachm, will produce almost immediate relief. I am in the habit of prescribing the above-named quantity at intervals of half an hour, until it produces its effects. In some instances, the dose requires no repetition. I think it much more prompt and certain in its effect in these cases than opium, and not so objectionable, being free of the constipating and stupefying properties of the latter. Its effects, of course, are to be ascribed to the hydrocyanic acid it contains, so that it is, perhaps, immaterial what form of the latter is used in such cases. The medicine was recommended in these cases by Hufeland.

December 24, 1860.

DOMESTIC SUMMARY.

Diphtheria.—Dr. FRANCIS MINOT gives (*Boston Med. and Surg. Journ.*, March 7, 1861) the following interesting account of the case of Dr. H. W. Adams, who died recently from an attack of diphtheria.

"On Tuesday, Feb. 5th, he [Dr. Adams] left Boston, in company with three other gentlemen, to pass a few days at Cotuit Port, a small village on the South Shore. He was suffering at the time from a heavy cold, and thought the change of air would be of benefit to him. The next day, one of the party, Mr. F. L. Gardner, 19 years old, a student in Harvard College, complained of a sore throat. His disease was considered and treated by Dr. Adams as one of acute tonsillitis, and appears to have been of moderate severity, as the patient was not confined to his bed. He had difficulty and pain in swallowing, swollen tonsils, and a whitish exudation on the fauces. On Sunday, Feb. 10th, Mr. Gardner was apparently better, and walked out, a short time, after breakfast. On returning to the house he expressed a wish to lie down, and Dr. Adams accompanied him to his chamber. He had hardly thrown himself upon the bed when he started up in a paroxysm of suffocation, and fell back dead. Dr. Adams immediately laid him on the floor, and tried to resuscitate him by clearing the throat with his fingers, and by endeavouring to inflate the lungs by blowing into his mouth. A large quantity of matter was removed from the patient's throat and mouth, and it was conjectured that an abscess in the tonsil had burst, and caused suffocation by the pus being inhaled. I may here remark, that this opinion was concurred in by Dr. J. Harpur, of Sandwich, who arrived after the patient's death. During their stay at Cotuit, the party experienced much vicissitude of weather,

the night between the 7th and 8th having been excessively cold, so that they all suffered, with the exception of Mr. Gardner, who was so well protected that he was not aware of its having been particularly cold.

The party returned to Boston on Monday, Feb. 11th, and the next evening Dr. Adams complained of some soreness of the throat, and passed an uncomfortable night, but on Wednesday, Feb. 13th, he was out, and attended the funeral of young Gardner. On his return home, however, he felt so unwell, that he sent for me, desiring me to bring some nitrate of silver and a sponge-probang. I saw him at about 2½ P. M. He was down stairs, walking about the room, rather excited, and the first thing he said was, "I have got young Gardner's disease." The voice was hoarse, the pulse at 108. The right side of the neck was a good deal swollen. Both tonsils were swollen, especially the right, and covered with a grayish-yellow, soft substance. The tongue was moist, and covered with a rather thick, dirty-white coat. The breath was rather offensive. There was no great difficulty in swallowing, and none in breathing. He had already taken some rhubarb, which had operated. He expressed a desire to go out, thinking that a walk would do him good. I persuaded him to go to his chamber, and in view of the circumstances of the case, proposed to ask Dr. Bowditch to see him with me, to which he readily assented. In the mean time, I directed him to take a grain of quinine every hour; strong soup every three hours, and brandy and water occasionally; and to gargle the throat with a solution of chlorate of potash, of the strength of half an ounce to a pint of water.

At 5 P. M., Dr. Bowditch saw him with me. He touched the fauces and epiglottis lightly with a sponge-probang dipped in solution of lunar caustic (thirty grains to the ounce), which was repeated in the evening, by his advice, with a solution of double the strength. He also advised a gargle of salt and vinegar, which, however, the patient found harsh, and did not continue. The treatment before adopted, in other respects, was continued. Dr. Adams was not at all prostrated, was cheerful, and said his throat was better.

Thursday, Feb. 14*th*.—Patient in bed. Dr. Bowditch saw him with me twice, and continued to do so daily, except on Saturday, 16th, when he saw him but once. To-day, the grayish appearance in the throat appeared extending, except towards the left side, and was invading the soft palate. The external swelling was increased; it was hard and lobulated, and but moderately tender. There was increased difficulty in swallowing. The uvula was large and œdematous, apparently filling up the narrow chink left between the tonsils. The pulse in the morning was at 96; towards evening it fell to 84. The voice generally whispering, but sometimes very hoarse. There was no difficulty of breathing; no nervousness; the patient was cheerful; the aspect and colour of the face were good. He was constantly employed in hawking and expectorating mucus, with a little serum, sometimes bloody. This symptom continued to the last—the favourite position of the patient being with his head over a basin at the side of his bed. During the day, he took a considerable quantity of beef-juice, milk and brandy, with quinine, but the pain in swallowing was evidently increased, and we were apprehensive that he would soon be unable to take nourishment by the mouth. Fearing that the greatly-enlarged uvula might accidentally obstruct the air-passage, by becoming engaged between the tonsils, it was amputated. It resembled a piece of umbilical cord, more than anything else.

Friday, Feb. 15*th*.—Dr. Adams was unable to swallow at all, during the night. He seemed unwilling to sleep, for fear he should be choked by the great amount of purulent mucus which accumulated in his throat. He however said he passed a comfortable night. Before morning he took some beef-juice and quinine in enema, which was not long retained. He afterwards had a free discharge from the bowels, after which he repeated the enema, with excellent effect, several times. He swallowed nothing during the day. The pulse was rather weaker, regular, and at about 84. There was less swelling outside the jaw on the right side. The right tonsil was much swollen, and with the soft palate adjoining, and the remains of the uvula, was covered with a grayish-white exudation. The breath was slightly offensive. The strength was good, and he was able to rise and walk about the room without effort.

Saturday, Feb. 16*th*.—The night was about the same as before. The patient

seemed to dread to sleep, and wished to be aroused should he do so. He dozed frequently, but had no continuous sleep. He succeeded in swallowing about half a pint of milk, mixed with bits of ice, during the night. The pulse in the morning was at 84; towards noon it rose to 96. The voice was fainter. There was at times some difficulty of breathing, but no actual dyspnœa. The patient was more restless than before. The throat looked more obstructed, the exudation extending up on the soft palate. At 4 P. M., he was suddenly seized with symptoms of strangling, and, after much effort, ejected a piece of firm, white membrane, five inches long, and one inch broad, which was tubular in two places. It was about the sixteenth of an inch thick, and looked like a piece of white kid. After this there was a coarse râle at times in the throat, but he could swallow with much more ease, and took freely champagne wine and beef-tea. At 7 P. M., he complained of great heat, and had the windows opened. The pulse at this time was at 108. He soon became more comfortable, but feeling anxious that he should have a medical man constantly with him, I requested Dr. Stearns to pass the night in his chamber, provided with instruments to open the trachea, in case symptoms of suffocation should come on. Dr. S. reported that he had a fair night. He raised two more pieces of membrane, smaller than the first, and without difficulty. The respiration was laboured at times, and he once rose and sat by the open window, feeling the want of air. He took about half a bottle of champagne, which he relished, and seemed to need. The pulse varied from 116 to 132. He arose once, and went to the water-closet without assistance. Dr. Stearns left him, apparently comfortable, at a quarter past 7, on the morning of Sunday, Feb. 17th. About twenty minutes afterwards, he suddenly had an attack of strangling, got out of bed, put his hands to his mouth, as if to remove some obstruction, and started for the door of an adjoining room. The nurse who was with him lifted him on the bed, and he immediately expired in the arms of his father, who entered the room at the moment. Dr. Stearns was summoned in haste, and opened the trachea, but although artificial respiration was kept up for some time, it was impossible to resuscitate him.

The following account of the *autopsy* was furnished by Dr. Ellis.

The neck was much swollen. The glottis and epiglottis were swollen and œdematous. A false membrane covered the tonsils and posterior part of the fauces, and extended through the glottis and trachea, to the secondary bronchi, where it terminated in a thin edge. It was somewhat ash-coloured, and perhaps less firm in the fauces than in the trachea. In the latter part it was quite firm, and about the sixteenth of an inch in thickness. It was perforated at the point of the operation, so that the air must have been admitted. Near the rima glottidis was a small shred, loose at one extremity, which might have caused obstruction, and suddenly terminated the life of the patient. The membrane was separated with ease from the subjacent surface, which was slightly reddened, and perhaps rough, but the change was not very marked. On microscopic examination of the false membrane, nothing unusual was observed. In the apex of the left lung were several groups of firm gray granulations. A few of the same were also seen at the right apex. Heart normal. Abdomen not exhibited."

Nitric Acid in Intermittent Fever.—Prof. WM. A. HAMMOND has published (*Maryland and Virginia Med. Journ.*, Feb. 1861) a table showing the results obtained in the treatment of forty-one cases of intermittent fever, at Fort Riley, Kansas Territory, in a period of six weeks in summer; of these forty-one cases, ten were quotidian and thirty-one tertian. Thirty-two cases were treated with the nitric acid and nine with the sulphate of quinine. Of the cases cured by nitric acid, three had previously used quinine without effect, and of those in which quinine had proved successful nitric acid had been employed without benefit in two, and in one other had to be omitted on account of causing nausea, heart-burn, &c.

The average period of treatment before the disease was permanently arrested was the same with each remedy—three days. The nitric acid was uniformly given in doses of ten drops (properly diluted with water) three times per day, the quinine in doses of eight grains three times per day.

Besides the fact that the nitric acid was equally successful with quinine in

arresting the disease, the difference in the cost of the two articles is so greatly in favour of the former substance, as to render it an object of importance to make its curative properties more widely known.

Nitric acid was first used as an anti-periodic by Dr. E. S. Baily, of Indiana. Its peculiar properties were brought to the notice of the profession by Dr. George Mendenhall, in the *Western Lancet* for August, 1854. A notice of the discovery is also contained in the *American Journal of the Medical Sciences* for October, 1854.

Dr. H. further states that he has since "very frequently employed nitric acid in the treatment of intermittent fever, and have rarely been disappointed in my expectations of its curative action. In fact, in simple uncomplicated intermittents, I seldom have occasion to use anything else.

"In cases of enlargement of the spleen, consequent upon frequent attacks of the ague, the remedy in question has, in my hands, proved very advantageous."

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Ice-Water in the Treatment of Croup.—Dr. J. A. McFarland invites (*Columbus Review*, Dec. 1860) the attention of the profession to the use of ice-water in the treatment of croup. He states that "more than seventeen years since he first employed cold applications in the management of croup. During the treatment of a most violent attack, after the ordinary means had been tried without any apparent benefit, and when fairly puzzled what next to do for the relief of the little sufferer, it occurred to my mind, on placing my hand over the child's throat, that the leading indication was to subdue the burning heat, the result of local inflammatory action, and that nothing could be better for that purpose than ice-water, applied directly over the inflamed parts. This was immediately done; and in ten or fifteen minutes a most wonderful improvement was evident. Never have I witnessed, in my whole professional experience, a more rapid and delightful change, than in this my first trial of ice-water in croup." He has since employed this in at least two hundred cases.

In applying the remedy he uses folds of muslin or linen large enough to cover the whole throat and upper part of the sternum, wet just sufficient to prevent dripping, and which should be well covered with several thicknesses of dry flannel, and the whole secured with a small handkerchief. "When we wish," he says, "to have the applications very cold, it is best to have two wet cloths, using them alternately, as fast as they become warm. This course should be continued till the disease is thoroughly subdued. When treatment is commenced early, a few hours may suffice; in neglected cases, several days are sometimes required. In some cases, water fresh from the well or cistern will be sufficiently cold; but as a general rule, the preference should be given to ice-water."

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Treatment of Rattlesnake Bite.—Dr. S. W. Mitchell has published (*N. A. Med.-Chir. Rev.*, March, 1861) an elaborate paper on this subject. The following is the course of treatment advised. "The disease caused by the venom is," he says, "sometimes so prompt and terrible that it is impossible to rouse the system through the stomach, and this is doubly difficult when vomiting becomes one of the prominent symptoms of the general prostration. Under these circumstances, enemata of brandy may be used, and inhalations of hot alcohol or ether resorted to, in order to re-excite the flagging powers.

"When called to a patient who has been bitten by a rattlesnake, the physician should at once ligate the limb with a *broad* band, as tightly as may be needed to check the circulation, while, wherever it is possible, cups should be also used immediately over the wound. The question of immediate excision or ablation of the part will then be determined by considerations already before the reader (local treatment). Setting these means aside, the iodine treatment, as limiting the local disease, may then be resorted to; but if, as is usually the case, there is no instrument at hand to make possible this treatment, incisions and the actually cautery are the final resort. Meanwhile, stimulus in some shape should be given, and when the excitement thus obtained is sufficient, the finger should be laid on the pulse and the band loosened. As the system becomes depressed, the ligature is once more to be drawn tighter, and, with continued use of stimulus,

the economy prepared for another dose of the venom, which is thus to be antagonized little by little. Finally, it will be requisite to shift the band higher up the limb, to avoid the too great constriction of the damaged member. The further management of the case, with regard to stimulus, must be left to the physician, who will remember that in most cases of severe poisoning, he has to deal finally with a blood which has lost a part or the whole of its power to coagulate. He may find in the mineral acids, tonics, as quinine, and the continued use of stimulus, the necessary means for carrying his patients through the later stages of the malady."

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Creasote as a Local Application in Diphtheria.—A correspondent of the *American Med. Times* (March 9, 1861) calls attention to the efficacy of creasote as a local application in diphtheria. "One or two applications," he says, "remove the fetor, and its continuance cures the throat." He employs ten or more drops of the creasote in a gill of warm water, as a gargle, or if the patient is too young for that, he applies it with a swab.

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Recurrent Sensibility of the Anterior Roots of the Spinal Nerves.—Dr. AUSTIN FLINT, JR., relates (*New Orleans Med. Times*, March, 1861) two experiments performed by him which show that the anterior roots of the spinal nerves are not exclusively motor, but that they possess a certain degree of sensibility; that this sensibility is recurrent, or is derived from the posterior or sensitive roots, because, after the division of these roots, it is immediately lost. In this he has confirmed the experiments of Magendie, in 1822 and 1839, experiments which he failed to repeat with success since that date, which were repeated in 1846 by Bernard, and later still by Schiff.

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Traumatic Tetanus; Inoculation with Corroval; Death.—Dr. E. T. MULHOLLAND records (*Maryland and Virginia Med. Journ.*, Jan. 1861) a case of traumatic tetanus in a coloured boy, in which various remedies having been administered without benefit, subcutaneous injection of corroval, at the suggestion of Dr. Hammond, was resorted to, but without producing the least impression on the tetanic spasms.

Prof. Hammond was of the opinion that no benefit is to be expected, in cases of this character, from the use of the corroval, and he supposed that death might have been indirectly hastened by the action of the substance in question upon the tissue of the heart. The mode, however, in which this event took place was sufficient to show that it was not due to the immediate effect of the corroval.

The case, he thought, was likewise instructive, as showing how radical a difference exists between traumatic tetanus and tetanus caused by over-doses of strychnia, and should prevent us forming conclusions as to treatment, applicable to both conditions.

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Oxalate of Cerium in the Vomiting of Pregnancy.—Dr. W. H. JONES reports (*Chicago Medical Journ.*, Feb. 1861) five cases in which he employed the oxalate of cerium in the vomiting of pregnancy, and with results entirely in accordance with those obtained by Dr. C. Lee. See his paper in the October No. for 1860 of this Journal.

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Tracheotomy in Croup.—Two cases of this, with successful results, are recorded in the *Boston Med. and Surg. Journ.*, Feb. 1861. The first is by Dr. JAS. AYER, of Boston. The subject of the case was a girl 5½ years of age, in whom the disease had run on for several days in spite of the usual remedies. Asphyxia being imminent, the child was etherized, and tracheotomy performed by Dr. Cabot, with instantaneous relief; and under very careful treatment the patient recovered.

The second case occurred in a boy two years and seven months old, and is related by Dr. SAMUEL CABOT, Jr. Suffocation being imminent, Dr. C. opened the trachea, and by careful subsequent treatment the child recovered.

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GRADUATES OF JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA, MARCH, 1861.

At a Public Commencement, held on the 9th of March, 1861, the degree of DOCTOR OF MEDICINE was conferred on the following gentlemen by the Hon. EDWARD KING, LL. D., President of the Institution; after which a Valedictory Address to the Graduates was delivered by Prof. MITCHELL.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Abernethy, James M.	North Carolina.	Enteric Fever.
Alger, Luther W.	Massachusetts.	Catarrhal Fever.
Alter, David	Pennsylvania.	Angina Pharyngæ.
Appel, Charles H.	Pennsylvania.	Intermittent Fever.
Armfield, David A.	North Carolina.	Scarlatina.
Atkins, T. W.	Georgia.	Crural Hernia.
Baker, John A. P.	Virginia.	Jaundice.
Balsbaugh, George S.	Illinois.	Scarlet Fever.
Barclay, John	Pennsylvania.	Enteric Fever.
Beckham, Henry C.	Virginia.	Dysentery.
Bell, John B.	Virginia.	Fœtal Circulation.
Bickley, Lloyd Wharton	Pennsylvania.	Inguinal Hernia.
Blackburn, Cary B.	Mississippi.	Hydrocele.
Blalock, N. G.	North Carolina.	Influence of the Mind over the Body.
Bowdon, P. M.	Mississippi.	Tubercular Phthisis.
Bowers, E. D.	Ohio.	Cervical Triangles.
Brothers, Oscar C.	Mississippi.	Acute Pneumonia.
Brown, Ignatius C.	Tennessee.	Pneumonia.
Brown, M. A.	Missouri.	Pinguedo.
Brunner, Frank R.	Pennsylvania.	Aims and Duties of the Medical Student.
Buford, Smith	Mississippi.	Parturition.
Burges, Richard U.	Virginia.	Enteric Fever.
Bush, Dudley	Kentucky.	The Organs of Vision.
Bushong, Israel	Pennsylvania.	Dropsy of the Knee.
Chambers, William D.	Kentucky.	Scarlatina.
Clark, Patrick B.	Texas.	Digestion.
Cochran, John L.	Virginia.	Connection between Mind and the Cerebral Hemispheres.
Coe, Thomas Upham	Maine.	Venesection.
Conkwright, Allen H. (M.D.)	Missouri.	Semeiology of Phthisis.
Conner, Phineas S.	Ohio.	General Paralysis of the Insane.
Cook, George R.	Florida.	Inflammation.
Covington, Thomas H.	Kentucky.	Gonorrhœa.
Crossley, George W.	Illinois.	Professional Distinction.
Culver, Lucius Pitt	Ohio.	Therapeutic Properties of Tartar Emetic.
Daniel, Thomas W.	Virginia.	Anæsthetics.
Davies, William H.	Virginia.	Pneumonia.
Davis, Nicholas E.	Virginia.	Angina Pectoris.
Delaney, William T.	Tennessee.	Diagnosis.
Duff, Edmund	Pennsylvania.	Diarrhœa.
Duke, James E.	Virginia.	Circulation of the Blood.
Dupuy, Joseph T.	Virginia.	Hectic Fever.
Dye, Henry	Texas.	Typhoid Fever.
Earnest, J. Tate	Tennessee.	Typhoid Fever.
Eaves, Spencer	North Carolina.	Laryngo-tracheitis.
Elder, Samuel M.	Pennsylvania.	Inflammation.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Embree, John W. (M. D.)	Texas.	Scurvy.
Engelman, Joseph P.	Pennsylvania.	Cholera Infantum.
Eves, Abram E.	Delaware.	Hygiene of the Sick-room.
Ewing, W. D.	Virginia.	Scarlet Fever.
Farley, John C.	Alabama.	Physical Signs of Pleuritis.
Finney, Edward B.	Virginia.	Childbed Fever.
Flint, John Felix	Pennsylvania.	Chagres Fever.
Fox, Addison C.	Virginia.	Our Profession.
Frantz, John H.	Maryland.	Ether and Chloroform as Anæsthetic Agents in Surgery.
Fulkerson, Albert P.	Missouri.	Acute Gastritis.
Gamble, Hamilton M.	Virginia.	Ilio-femoral Luxations.
Garrett, Lewis T.	Pennsylvania.	Scarlatina.
Gidney, J. Chauncey	North Carolina.	Dysentery.
Gilman, Uriah	Arkansas.	Cathartics.
Goolrick, P., Jr.	Virginia.	Medical Science.
Grant, John (M. D.)	Canada West.	Pneumonia.
Greene, Lorenzo S. S.	Alabama.	Oxygen as a Chemico-physical Agent.
Gross, Chester L.	Pennsylvania.	Signs of Pregnancy.
Hammond, Alfred F.	North Carolina.	Bilious Fever.
Hanger, Cornelius	Missouri.	Malaria.
Hanly, John A. C.	Pennsylvania.	Typhoid Fever.
Harrill, Lawson	North Carolina.	Laryngismus Stridulus.
Haughton, Rich'd E. (M.D.)	Indiana.	Diphtheria.
Haupt, Frederick L.	Pennsylvania.	Typhoid Fever.
Hawkins, John W.	Missouri.	The Mind in Disease.
Henry, David H.	Indiana.	Mania-a-potu.
Herr, Ambrose J.	Pennsylvania.	Chorea.
Herrick, Geo. H. Webster	New Hampshire.	Death.
Hetzell, David G.	New Jersey.	Anatomy and Physiology of the Skin.
Higgins, Samuel J.	Georgia.	Scarlatina.
Hill, Lauriston H.	North Carolina.	Tobacco.
Hitch, William S.	Delaware.	Enteric Fever.
Horton, Samuel M.	Pennsylvania.	Germinal Spots.
Howard, Thomas Henry	Virginia.	Intermittent Fever.
Hugg, Joseph	New Jersey.	Dysentery.
Jones, E. W.	Alabama.	Influenza.
Jones, La Fayette J.	Virginia.	Menstruation.
Keely, Thomas J.	Pennsylvania.	Pseudo-membranous Angina.
Keene, Robert Wilson	Kentucky.	Marriages of Consanguinity.
King, William Coffield	Tennessee.	Enteric Fever.
Knott, Thomas M.	Kentucky.	Inflammation.
Kuykendall, William C.	Mississippi.	Dysentery.
Lane, Sidney W.	Maryland.	Diphthérite.
Lea, John G.	North Carolina.	Diphtheria.
Lineaweaver, John K.	Pennsylvania.	Dysentery.
Logan, P. W.	Kentucky.	Clinical Examination of Patients.
Lowry, Squire M.	Kentucky.	The M. D.'s Mission.
Marley, H. B.	North Carolina.	Gunshot Wounds.
Marsh, Joseph W.	Delaware.	Typhoid Fever.
Martin, James	Ohio.	Pseudo-membranous Croup.
Massie, James W. (M. D.)	Kentucky.	Counterirritation.
Mathis, Aylesbury	Georgia.	Cyanosis Neonatorum.
McClarty, Hugh G.	Texas.	Gunshot Wounds.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
McGee, J. P.	Tennessee.	Retrospect of Medicine.
Metheny, David	Pennsylvania.	The Physician.
Michler, William H. H.	Pennsylvania.	Tussis Convulsiva.
Miller, Victor Davis	Pennsylvania.	Veratrum Viride.
Milloy, John	Mississippi.	Enteric Fever.
Miner, James	Illinois.	Enteric Fever.
Montanye, Lester de la	Pennsylvania.	Tubercular Phthisis.
Morley, J. R.	Tennessee.	Typhoid Fever.
Morris, John	Ohio.	Gonozæmia.
Morrison, Joseph B.	Pennsylvania.	Dropsy.
Moses, Thomas Freeman	Maine.	Modes of Death.
Mosley, Benjamin J.	Georgia.	Sunstroke.
Munford, Samuel E.	Indiana.	Chemistry applied to Therapeutics and Forensic Medicine.
Nall, Burr F.	Kentucky.	Phthisis Pulmonalis.
Nicholson, John (M. D.)	Pennsylvania.	Phthisis Pulmonalis.
Noble, James D.	Pennsylvania.	Anæsthetics.
Norman, John P.	Pennsylvania.	Bronchitis.
Notson, William Morrow	Pennsylvania.	Diphtheritis.
Nottingham, Severn P.	Virginia.	Delirium Tremens.
O'Bryan, William R.	Kentucky.	Post-partum Hemorrhage.
Oldmixon, George Scott	Pennsylvania.	Diagnosis of Inguinal Hernia.
Parker, D. Reid	North Carolina.	Intermittent Fever.
Parker, William H. H.	Virginia.	Hydrophobia.
Pease, Loren H.	Connecticut.	Sumach.
Picot, Mitchell H.	Pennsylvania.	Typhoid Fever.
Pomerene, Peter P.	Ohio.	Diphtheria.
Powell, H. B.	Ohio.	Typhoid Fever.
Price, William Frederick	Virginia.	Typhoid Fever.
Pulliam, John D.	Virginia.	Digestion.
Pyatt, E. A.	Tennessee.	Scrofula.
Quarterman, Keith A.	Georgia.	Intermittent Fever.
Ragsdale, Joseph	Missouri.	Typhoid Fever.
Rhinehart, Alexander K.	Georgia.	Puerperal Convulsions.
Rhoads, Thomas J. B.	Pennsylvania.	Axillary Dislocations.
Rice, Albert R.	New York.	Retroversion of the Uterus.
Richey, James A.	Pennsylvania.	Diuretics.
Riggs, David W.	Pennsylvania.	Scarlet Fever.
Ritter, Nathaniel F.	Pennsylvania.	Typhoid Fever.
Robbins, Joseph	Illinois.	Periodicity in Disease.
Roberts, William H.	Indiana.	Pneumonia.
Robertson, A. T.	Arkansas.	Intermittent Fever.
Robertson, T. L.	Alabama.	Menstruation.
Roller, William C.	Pennsylvania.	Phenomena of Inflammation.
Ross, Elijah W.	Pennsylvania.	Chronic Cystitis.
Rugeley, Henry L.	Texas.	Abdominal Dropsy.
Sanders, William H.	Alabama.	Inguinal Hernia.
Satterfield, Benjamin F.	Missouri.	Syphilis.
Saunders, John B.	Kentucky.	Pathology and Treatment of Scarlatina.
Savage, William E. F.	Virginia.	Laryngo-tracheitis.
Seargeant, Henry H.	Virginia.	The Signs of Pregnancy.
Senseny, William D.	Pennsylvania.	Diphtheria.
Shackelford, William	Kentucky.	Hydrocele.
Shackleford, James	Kentucky.	Syphilis.
Shankle, E. A.	Georgia.	Scarlatina.
Short, Wesley	Indiana.	Milk Sickness.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Simmons, Thomas W.	Maryland.	Diphtheria.
Simpson, Joseph Hawkins	North Carolina.	Typhoid Fever.
Slough, G. B.	Pennsylvania.	Physic, its true Nobility.
Smoot, John H.	Virginia.	Opium.
Spencer, William Canfield	New York.	Investigation in Diagnosis.
Stewart, Samuel Shaw	Pennsylvania.	Polygala Senega.
Stowe, Charles H.	Pennsylvania.	Aneurisms.
Thomas, Richard C.	Kentucky.	The Kidney.
Thompson, Thomas C.	Texas.	Pulmonitis.
Thompson, William B.	Pennsylvania.	Laws which govern the Strength of a Galvanic Current.
Thomson, James W.	Pennsylvania.	Variola.
Totten, John Baldwin	Ohio.	Fœtal Circulation.
Trego, Albert	Pennsylvania.	Diphtheria.
Tyson, Andrew R.	Pennsylvania.	Placenta Prævia.
Vaughan, Joseph H.	Virginia.	Chorea.
Wailes, Leonard A.	Mississippi.	Strabismus.
Wallis, Walter	Maryland.	Pneumonia.
Warren, Francis G. (M. D.)	Maine.	Enteric Fever.
Washington, James S.	Arkansas.	Auscultation and Percussion.
Way, William Henry	Georgia.	Bilious Remittent Fever.
Webb, Charles E.	Virginia.	Erysipelas.
Weist, J. R.	Ohio.	Duties of the Physician.
Welch, Stanton A.	Pennsylvania.	Retroversion of the Uterus.
White, Horace M.	Pennsylvania.	Scrotal Hernia.
Williams, Emmet	Mississippi.	Hydrophobia.
Williams, Junius S.	North Carolina.	Circulation of the Blood.
Willson, John	Pennsylvania.	Antimonial Powder.
Wilson, Lucien Strain	Georgia.	Diphtheria.
Wilson, W. Stockton	Maryland.	The Health of our Women.
Windle, Isaac	Iowa.	Inflammation of the Cervix Uteri.
Wood, J. Bestor	Alabama.	Menstruation.
Woolsey, William F.	Pennsylvania.	Diphtheria.
Wortham, John B.	Alabama.	Dysentery.
Wymond, Richmond	Indiana.	Diphtheria.

Of the above, there are from—

Pennsylvania	47	Illinois	4
Virginia	26	Arkansas	3
Kentucky	14	Delaware	3
North Carolina	13	Maine	3
Georgia	9	New York	2
Ohio	9	New Jersey	2
Mississippi	8	New Hampshire	1
Alabama	7	Florida	1
Tennessee	7	Connecticut	1
Missouri	7	Iowa	1
Texas	6	Massachusetts	1
Indiana	6	Canada West	1
Maryland	5		
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Philadelphia, Pa.

GRADUATES OF THE UNIVERSITY OF PENNSYLVANIA, 1860.

At a Public Commencement, held March 14th, 1861, in the Musical Fund Hall, the Degree of Doctor of Medicine was conferred by Rev. DANIEL R. GOODWIN, D.D., Provost, upon the following gentlemen; after which an Address was delivered by ROBERT E. ROGERS, M. D., Professor of Chemistry.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Allen, Harrison	Philadelphia,		Pa.	Entozoa Hominis.
Alexander, Octavius	Lexington,	Lafayette,	Mo.	Life of the Blood.
Amiss, Thos. B.	Slate Mills,	Rappahannock,	Va.	Typhoid Fever.
Armstrong, N. E.	Hallsville,	Duplin,	N. C.	Pleuritis.
Armstrong, James A.	Philadelphia,		Pa.	Children and their Diseases
Ashhurst, Samuel	Philadelphia,		Pa.	Periodical Dyspnœa.
Bacon, Charles P.	Roaring Spring,	Trigg,	Ky.	Causa Excitans Parturitiones.
Bacon, Thos. L. (M.D.)	Mayfield,	Graves,	Ky.	Milk Sickness.
Baldwin, C. (M.D.)	Winchester,	Frederick,	Va.	Signs of Pregnancy.
Bellows, Horace M.	Philadelphia,		Pa.	Wounds of the Abdomen.
Beshler, J. B.	Berrysburg,	Dauphin,	Pa.	Diphtheria.
Birkey, Henry W.	Philadelphia,		Pa.	Fractures.
Bonner, Robt. J.	Raymond,	Hinds,	Miss.	Yellow Fever.
Booth, E. G., Jr.	Wellville,	Nottoway,	Va.	Diagnosis and Treatment of Enteric Fever.
Bowen, Jno. B.	Bridgton,	Cumberland,	N. J.	Belladonna.
Brengle, Wm. D.	Frederick City,	Frederick,	Md.	Pseudarthrosis.
Breneman, E de W.	Lancaster,	Lancaster,	Pa.	Osteosis.
Bridges, John H.	Taylortown,	Sunbury, N. Brunswick,		Croup.
Brown, Jas. W. (M.D.)	Amherst C. H.,	Amherst,	Va.	Gleet.
Brown, A. M.	Cincinnati,	Hamilton,	Ohio.	Correlation of Forces.
Bunn, Wm. H.	Philadelphia,		Pa.	Acute Tetanus.
Cadwalader, Chas. E.	Philadelphia,		Pa.	Dyspepsia.
Cameron, Hugh	Antigonishe,	Sydney, Nova Scotia,		Pleurisy.
Carter, C. Shirley	Millwood,	Clark,	Va.	Pneumonia.
Casper, Thos. J.	Salem,	Salem,	N. J.	Hydrargyrum.
Chandler, J. T.	Oxford,	La Fayette,	Miss.	Yellow Fever.
Cheatwood, L. N.	Lynchburg,	Campbell,	Va.	Labour.
Cheek, B. A.	Warrenton,	Warren,	N. C.	The Pulse as a Diagnostic Symptom.
Church, Wm.	Meadville,	Crawford,	Pa.	Monomania.
Clark, Dougan	Westfield,	Hamilton,	Ind.	Parturition and its Management.
Clark, R. Woodhull	Philadelphia,		Pa.	Signs of Pregnancy.
Clemens, Henry S.	Springtown,	Bucks,	Pa.	Pneumonia.
Cobb, Wm. H. H.	Goldsboro',	Wayne,	N. C.	Lobelia Inflata.
Collins, Wm. A.	Vicksburg,	Warren,	Miss.	Phenomena of Inflammation.
Craighill, Edw. A.	Charlestown,	Jefferson,	Va.	Yellow Fever.
Crain, Wm. Baker	Warren,	Herkimer,	N. Y.	Croup.
Cruice, John J.	Philadelphia,		Pa.	Puerperal Convulsions.
Darrach, Wm., Jr.	Philadelphia,		Pa.	Diphtheria.
Davis, John I.	Buckeystown,	Frederick,	Md.	Delirium Tremens.
Davis, J. D.	Prairieville,	Pike,	Mo.	Jaundice.
Detwiler, S. B.	West Earl,	Lancaster,	Pa.	The Character and Duty of a Physician.
Dodson, M. Emmet	Greensborough,	Guilford,	N. C.	Puerperal Fever.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Dungan, D. H.	Thompson,	Williamson,	Tenn.	Topography & Diseases of vicinity of Thompson, with remarks on Infantile Tetanus and Synochoid Fever.
Dunlap, F. S.	Delaware City,	New Castle,	Del.	Experiments on the effects of penetrating Wounds of the Chest.
Ellis, Geo. W.	Philadelphia,		Pa.	Odontalgia.
Ellis, Chas. M.	Elkton,	Cecil,	Md.	Diphtheria.
Evans, James	Pine Bluffs,	Jefferson,	Ark.	Dyspepsia.
Eversfield, W. Octavus	Bladensburg,	Pr. George,	Md.	Malaria.
Ford, Chas. M.	Troy,	Rensselaer,	N. Y.	Scarlatina.
Forwood, Wm. H.	Chester,	Delaware,	Pa.	Differential Diagnosis of the Exanthemata.
Fowler, Geo. H.	Mobile,	Mobile,	Ala.	Parturition.
Frick, A. P.	Neffsville,	Lancaster,	Pa.	The Twofold Nature of Man.
Fuller, Junius T.	Berea,	Granville,	N. C.	Enteric Fever.
Fulton, J. Harvey	Elkton,	Cecil,	Md.	Labiate and Pycnanthemum Linifolium.
Gardner, T. Sobieski	Hollidaysburg,	Blair,	Pa.	The Venereal Disease.
Garsia, W. Marston	Valparaiso,	South America,		Medico-Chemical Analysis of the Urine.
Ginkinger, W. H. H.	Allentown,	Lehigh,	Pa.	Enteric Fever.
Gleason, M. K.	Summit,	Cambria,	Pa.	Sulphate of Quinia.
Gotwald, J. H.	Cincinnati,	Hamilton,	Ohio,	Mucous Membrane Anatomically, Physiologically, and Pathologically Considered.
Gregory, F. R.	Sassafras Fork,	Granville,	N. C.	Semeiology of Pregnancy.
Grier, J. H.	Elizabeth,	Union,	N. J.	Remittent Fever.
Haney, J. Reigel	Doylestown,	Bucks	Pa.	Acute Pneumonitis.
Hardaway, Daniel H.	Nottaway C. H.,	Nottaway,	Va.	Hydrophobia.
Harris, J. O.	Washington,		D. C.	Delirium Tremens.
Harris, R. B. P.	Milldale,	Warren,	Miss.	Acute Dysentery.
Hawkins, A. W. H.	Clarksville,	Washington,	Pa.	Dyspepsia.
Hay, Thomas	Philadelphia,		Pa.	Acute Pleurisy.
Henderson, N. P.	Franktown,	Northampton,	Va.	Croup.
Herron, Jas S.	Washington,	Escambia,	Fla.	Typhus Icterodes.
Hersom, Nahum A.	North Lebanon,	York,	Maine,	Hereditary Transmission.
Hicks, J. H.	Faisons,	Duplin,	N. C.	Acute Pneumonia.
Higgins, J. Mitchell	Lambertsville,	Hunterdon,	N. J.	Duties of the Accoucheur in Natural Labour.
Holt, W. A.	Graham,	Alamance,	N. C.	Cholera Infantum.
Hooks, J. F.	Boston,	Bowie,	Texas,	Sleep.
Horn, Geo. H.	Philadelphia,		Pa.	Sprains.
Hunter, H. T.	Kempersville,	Pr. Anne,	Va.	Typhoid Fever.
Hurst, M. W.	Goodville,	Lancaster,	Pa.	Typhoid Fever.
Illig, A. F.	Stouchburg,	Berks,	Pa.	Strabismus.
Jamar, J. H.	Elkton,	Cecil,	Md.	Gelsenimum Sempervirens.
James, Ed. C.	Courtland,	Lawrence,	Ala.	Pneumonia.
Johnson, J. T.	Happy Home,	Burke,	N. C.	Duty of a Medical Student.
Joyce, E.	Baltimore,		Md.	Dyspepsia or Indigestion.
Knight, Jas. S.	Camden,	Kent,	Del.	Sunlight.
Knight, M. D.	Montg Square,	Montgomery,	Pa.	Hæmatemesis.
Kohler, John P.	White Hall Station,	Lehigh,	Pa.	Scarlatina.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Landis, H., Jr.	Lancaster,		Pa.	Hereditary transmission of Disease.
Leete, J. M.	Calhoun Depot,	Madison,	Miss.	Parenchymatitis Renalis.
Lesesne, Chas.	Elizabethtown,	Bladen,	N. C.	Pneumonia.
Levan, Jeremiah R.	Kutztown,	Berks,	Pa.	Pneumonia.
Love, W. W.	Quincy,	Gadsden,	Fla.	Physical, Chemical, and Structural Characters of the Blood.
Marcy, Alex.	Cape Island,	Cape May,	N. J.	The Effects of Strychnia locally upon Inflammation.
Marriott, Henry	West River,	A. Arundel,	Md.	Diabetes.
Mays, George	Schafferstown,	Lebanon,	Pa.	The Blood.
McCausland, R. K.	Pt. Pleasant,	Mason,	Va.	Diphtheritis.
McCloud, H. C.	Robson's Landing,	Coahoma,	Miss.	Erysipelas.
McGill, John	Clio,	Marlboro',	S. C.	Typhoid, or Enteric Fever.
McGill, Geo M.	Princeton,	Mercer,	N. J.	The Liver.
McPherson, Samuel M.	Lewisburg,	Greenbrier,	Va.	Diphtheria.
Meade, H. B.	Chula,	Amelia,	Va.	Man and Woman.
Medlin, P. P.	Eagle Rock,	Wake,	N. C.	Remittent Fever.
Mentzer, Wm.	Carlisle,	Cumberland,	Pa.	Veratrum Viride.
Meredith, J. Q. A.	Pughtown,	Chester,	Pa.	Wounds.
Millar, Robt.	Providence,	Providence,	R. I.	Diphtheria.
Miller, Jacob A.	Lancaster,	Lancaster,	Pa.	Bloodletting
Miller, Jesse	Smithfield Station,	Mahoning,	Ohio.	Pleuritis.
Mitchell, Geo. H.	Philadelphia,		Pa.	Phenomena of Psychical Changes.
Mitchell, Goodrich	Warrenton,	Fauquier,	Va.	Mercury.
Morrison, T. S.	Coatesville,	Chester,	Pa.	Scarlatina.
Morrison, W. Clingan	Philadelphia,		Pa.	Enteric Fever.
Morrison, J. A. (M. D.)	Philadelphia,		Pa.	
Murphy, Robt. R.	Unionville,	Union,	S. C.	Intermittent Fever.
Nassau, R. Hamill	Lawrenceville,	Mercer,	N. J.	De Officiis Adipis.
Noble, P. H. C.	Morven,	Amelia,	Va.	Typhoid Fever.
Norris, Wm. F.	Philadelphia,		Pa.	Generation and Development.
Nowland, Jas. A.	New Castle,		Del.	Hysteria.
Nuttall, J. H.	Memphis,	Shelby,	Tenn.	Diphtheria.
Oakley, Thos. A.	Leasburg,	Caswell,	N. C.	Abortion.
O'Neill, James P.	Philadelphia,		Pa.	Cathartics.
Opie, Thomas	Staunton,	Augusta,	Va.	Opium.
Page, R. P. (M. D.)		Clarke,	Va.	Hearing.
Parry, H. Chester	Pottsville,	Schuylkill,	Pa.	Opium.
Paullin, Geo. M.	West Philada.,		Pa.	Inflammation.
Perkins, Edw. W.	Malonesville,	Dinwiddie,	Va.	Intermittent Fever.
Pulliam, A. B.	La Grange,	Fayette,	Tenn.	Gonorrhœa.
Ramsey, J. Sylvester	Bloomsburg,	Columbia,	Pa.	Diabetes Mellitus.
Reeder, Silas Andrews	Doylestown,	Bucks,	Pa.	Intermittent Fever.
Reid, Thos. J.	Macon,	Fayette,	Tenn.	Anæsthesia in Parturition.
Rewalt, Luther L.	Middletown,	Dauphin,	Pa.	Yellow Fever.
Robbins, Harry A.	Washington,		D. C.	The Uterus.
Robertson, J. B.	Creachville,	Johnston,	N. C.	Enteric Fever.
Robertson, S. D.	Brownsville,	Hinds,	Miss.	Pneumonia.
Robinson, Henry	Laurens C. H.	Laurens Dist.,	S. C.	Etiological and Therapeutical Effects of Emotions and Passions.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Roe, Jos. B.	Philadelphia,		Pa.	Puerperal Peritonitis.
Rogers, M. L.	Knoxville,	Knox,	Tenn.	Erysipelas.
Rupp, Harry B.	Mechanicsb'g,	Cumberland,	Pa.	Licentiousness—Its moral and Physical Effects.
Russ, Simpson	White Creek,	Bladen,	N. C.	Acute Pneumonia.
Savery, Wm.	Philadelphia,		Pa.	Abortion.
Scull, W. J.	Marianna,	Jackson,	Fla	Pernicious Fever.
Seawell, L. H.	Marion,	Perry,	Ala.	Dysentery.
Shank, John H.	Lebanon,	Lebanon,	Pa.	Rheumatic Pericarditis.
Shepherd, Cornelius, Jr.	Doylestown,	Bucks,	Pa.	Bile and its Uses.
Shimer, Jacob C.	Easton,	Northampton,	Pa.	The Physical Education of the Female.
Sloan, Chas. J.	Brownsville,	Fayette,	Pa.	Delirium Tremens.
Smith, Wm. F.	Chanceford,	York,	Pa.	Gastronomy.
Sowerby, J. John	Holmesburg,	Philadelphia,	Pa.	Phthisis.
Spear, John C.	Middletown,	New Castle,	Del.	The Blood.
Stedman, H. C.	Milton,	Northumber'd,	Pa.	Signs of Pregnancy.
Stephens, W. G.	Leasburg,	Caswell,	N. C.	Scarlatina.
Stephenson, J. M.	Parkersburg,	Wood,	Va.	Chancre.
Stovall, J. B. (M. D.)	Sassafras Fo'k,	Granville,	N. C	Scarlatina.
Strickler, M. B.	Carlisle,	Cumberland,	Pa.	Chorea.
Stroud, P. Van Buren	Parkesburg,	Chester,	Pa.	Typhoid, or Enteric Fever.
Swasey, Chas. E.	Milton Mills,	Strafford,	N. H.	Major Exanthemata.
Talbot, John W.	Boston,	Bowie,	Texas,	Puerperal Fever.
Timms, C. W.	Buffalo,	Putnam,	Va.	Pleurisy.
Trau, Adam	Philadelphia,		Pa.	Nephrolithiasis.
Tryon, J. Rufus	New York,		N. Y.	Excretion.
Vallarino, B.	Panama,	Panama,	S. Am.	Dyspepsia.
Voorhies, Charles A.	Easton,	Northampton,	Pa.	The Duties of a Young Physician as essential to his Progress in the Profession.
Wager, P.	Mont. Square,	Montgomery,		Scorbutus.
Wayt, Newton	Staunton,	Augusta,	Va.	Pneumonia.
Wales, Philip S. (M. D.)	U. S. N.			Dysentery.
Ware, John J.	Brownsville,	Haywood,	Tenn.	Remittent Fever.
Watford, Wm. B.	Pitch Landing,	Hertford,	N. C.	Portal Circulation.
Watson, Wm.	Bedford,	Bedford,	Pa.	Enteric Fever.
Weaver, John D.	Danville,	Montour,	Pa.	Diseases of Pregnancy.
Whilldin, J. Stites	Philadelphia,		Pa.	Erysipelas.
Wiestling, R. R.	Middletown,	Dauphin,	Pa.	Acute Rheumatism.
Williams, Monroe	Lumpkin,	Stewart,	Ga.	Typhoid Fever.
Williams, V. O.	Moorefield,	Hardy,	Va.	Enteric Fever.
Wolfley, Wm. Irvin	Newport,	Campbell,	Ky.	The Surgeon's Means of regulating the Inflammatory Process.
Worthington, R. H. Jr.	Murfreesboro',	Hertford,	N. C.	Modifying Influences of Miasmata on Enteric Fever.
Yarrow, Thos. J.	Allowaystown,	Salem,	N. J.	Digitalis Purpurea.
Yarrow, H. C.	Philadelphia,		Pa.	Urine and Urinary Deposits.
Young, John	Philadelphia,		Pa.	Variola.

The Lectures of the ensuing Session of 1861–62 will begin on the second Monday of October and close on the first of March.

HARVARD UNIVERSITY.

SUMMER SESSION OF THE MEDICAL DEPARTMENT.

THE Annual Course of Summer Instruction in the Medical Department of Harvard University will commence at the Massachusetts Medical College, in North Grove Street, Boston, on Monday, March 11, 1861, and continue till November.

Clinical Medical and Surgical Instruction will be given at the Massachusetts General Hospital, adjoining the College.

Recitations from approved text-books will be held daily during the session at the College, upon all branches necessary to a medical education. Occasional lectures are also given, and demonstrations, illustrated by the Museums of the College.

During the Summer Session, instruction is given by lectures at Cambridge, on Botany, by Prof. Gray; on Comparative Anatomy, by Prof. Wyman; on Zoology, by Prof. Agassiz; on Acoustics and Optics, by Prof. Lovering. To these lectures, students of the Summer Session will be admitted without extra charge.

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D. HUMPHREYS STORER, M. D., Professor of Obstetrics and Medical Jurisprudence.
JOHN B. S. JACKSON, M. D., Professor of Morbid Anatomy.
HENRY I. BOWDITCH, M. D., Professor of Clinical Medicine.
OLIVER W. HOLMES, M. D., Professor of Anatomy and Physiology.
GEORGE C. SHATTUCK, M. D., Hersey Professor of the Theory and Practice of Medicine.
HENRY J. BIGELOW, M. D., Professor of Surgery and Clinical Surgery.
JOHN BACON, M. D., Professor of Chemistry.
EDWARD H. CLARKE, M. D., Professor of Materia Medica

DAVID W. CHEEVER, M. D., Demonstrator and Assistant in Surgery.

WM. E. COALE, M. D., Assistant in Theory and Practice.
FRANCIS MINOT, M. D., Assistant in Theory and Practice.
CALVIN ELLIS, M. D., Assistant in Morbid Anatomy.
J. NELSON BORLAND, M. D., Assistant in Clinical Medicine.
JAMES C. WHITE, M. D., Assistant in Chemistry.
E. OLIVER FITCH, M. D., Assistant in Materia Medica.

Tickets to the Session must be procured before students will be admitted to the Course. D. HUMPHREYS STORER, Dean of the Faculty,
Jan. 1, 1861.—2t *No. 132 Tremont Street, Boston.*

Circulars can be obtained gratis, upon application to David Clapp, Medical and Surgical Journal Office, over 184 Washington Street, Boston.

UNIVERSITY OF MARYLAND.

SCHOOL OF MEDICINE.

THE Fifty-fourth Session of this institution will begin on Monday, October 14, 1861, and end March 1, 1862.

FACULTY.

NATHAN R SMITH, M. D., Professor of Surgery.
WILLIAM E. A. AIKIN, M. D., Professor of Chemistry and Pharmacy.
SAMUEL CHEW, M. D., Professor of Principles and Practice of Medicine, and of Clinical Medicine.
G. W. MILTENBERGER, M. D., Professor of Obstetrics and the Diseases of Women and Children.
WILLIAM A. HAMMOND, M. D., Professor of Anatomy and Physiology.
EDWARD WARREN, M D., Professor of Materia Medica and Therapeutics.

JAMES H. BUTLER, M. D., Demonstrator of Anatomy.

Since their last annual announcement the Faculty have very materially added to their already ample means for giving medical instruction. The Baltimore Infirmary, which contains 150 beds, and is entirely under their direction, is open throughout the year, free of charge to the matriculants of the School. Clinical instruction is given

UNIVERSITY OF MARYLAND—CONTINUED.

at this institution four days in each week of the session, and this instruction is continued daily the rest of the year. The advantages, therefore, which the University affords for obtaining a practical knowledge of Medicine and Surgery are unequalled by any other Medical School of the country.

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G W. MILTENBERGER, M. D,
Dean of the Faculty.

8t.

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ON BANDAGING AND OTHER OPERATIONS OF MINOR SURGERY.

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Member of the College of Physicians of Philadelphia, *&c.*

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